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ORIGINAL ARTICLES.

I.—*Hapalocrinus Victoriæ*, n.s., Silurian, Melbourne, and its relation to the Platycrinidæ.

By F. A. Bather, M.A., F.G.S.

(PLATE XV.)

Description of the Type-specimen.

THE specimen herein described was sent me by Mr. T. S. Hall, M.A., of Melbourne University. It was found by Mr. F. P. Spry in a rock of supposed Silurian age, at the Yarra Improvement Works, near Prince's Bridge, Melbourne, in November, 1896. The matrix is a fine-grained micaceous sandstone, of a dark purple-grey, weathering yellowish. It often contains crinoid columnals, such as were long ago noticed by the Geological Survey of Victoria,[1] and were figured by W. Blandowski as "*Cyatocrinites* (probably) *pinnatus*"[2]; but neither from here, nor from any other Silurian rock of Australia, has a crinoid crown hitherto been recorded. The remains of the crinoid are in the form of an impression, coloured red by iron-oxide; all calcareous matter has been dissolved away. The counterpart of the impression has not been preserved. The specimen was studied by means of wax squeezes; the drawing (Plate XV)[3] represents such a squeeze; and the description follows the drawing.

The crinoid, regarded thus, is seen to lie with its ten arm-branches fairly regularly extended, having their ventral grooves downwards; the dorsal cup, which would naturally lie base upwards, is bent over so that its apex or base points towards the bottom of the drawing; no trace of stem remains.

The Dorsal Cup appears to consist of two circlets of plates, basals and radials.

The number and shape of the Basals cannot be seen clearly. Apparently these plates were small; the non-appearance of sutures suggests that they were not more than three in number.

[1] A. R. C. Selwyn, "On the Geology of the Gold-fields, etc.": Quart. Journ. Geol. Soc., xiv, 533–8, 1858.

[2] "A Description of Fossil Animalculæ in Primitive Rocks from the Upper Yarra District": Trans. Phil. Soc. Victoria, i, 221–3, with plate, 1855. See figs. 309–14 and 361.

[3] *Hapalocrinus Victoriæ*, from a wax squeeze of the unique type-specimen, × 5 diameters. Owing to the imperfection of the specimen, every single line in this cannot be vouched for; but the general relations and the details in the immediate neighbourhood of the calyx are believed to be correctly represented.

The Radials, five in number, appear to have been much wider above than below; but the intra-radial sutures are not clear, and the appearance is no doubt due in part to the flattening of the cup. The facet for the arms occupies about one-third the width of the radial.

The TEGMEN can be dimly seen, stretching up between the arms, to a height about equal to that of the dorsal cup. It appears to join on to the primibrachs, but is enough depressed between the arms and distinct from the radials to be considered no part of the dorsal cup. In other words, while the crinoid is not Inadunate, neither is one of the highly developed Camerata. The number and arrangement of the tegminal plates is not to be distinguished.

The height of the calyx from its base to the top of the tegmen is about 3 mm.

The 5 ARMS each branch once, on IBr_3; the total length is 15 mm.

The 3 Primibrachs (IBr) are joined to the tegmen as far up as the lower part of IAx. Their width is about ·5 mm.; the length about half as much again.

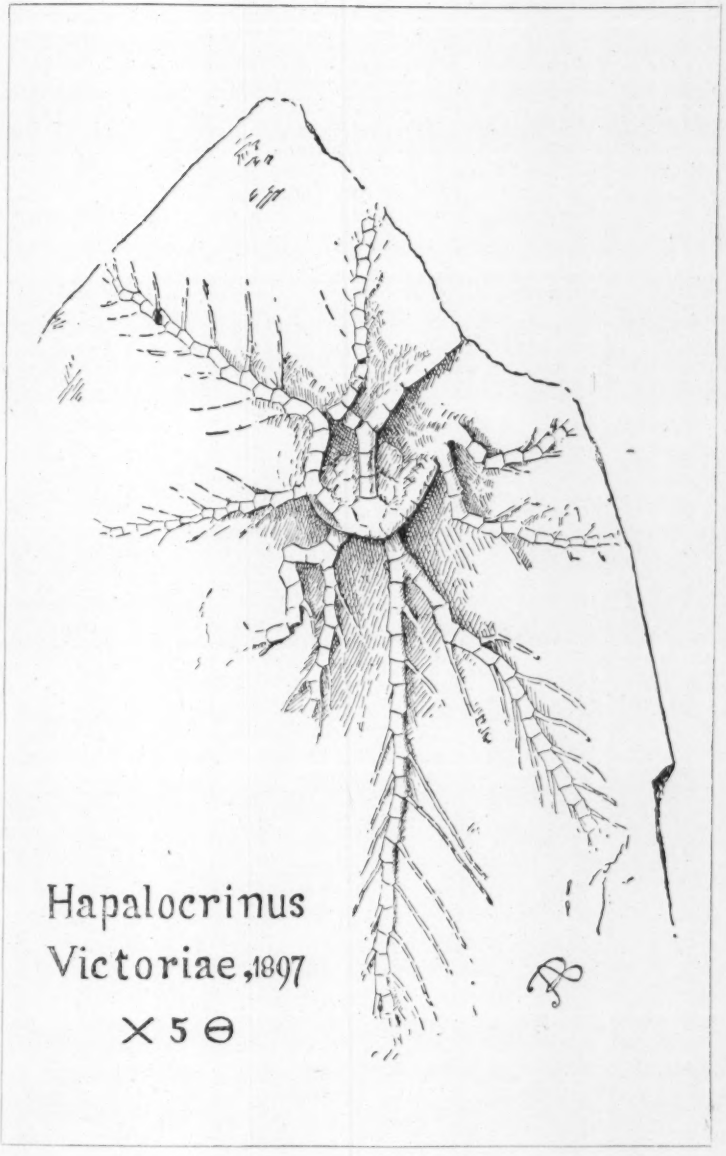
The Secundibrachs (IIBr), which begin by being slightly less wide than the IBr, are about half as long again as wide. The width gradually decreases to the end of the arm.

Pinnules, alternating from side to side of the arm-branch, are borne by each IIBr, except the first, and perhaps the second. The facets for the pinnules are well marked, so that the long axes of the IIBr form a slightly zigzag line. The greatest length of pinnule observed is 4·75 mm., its width being ·125 mm. The pinnulars appear to have been long.

THE SILURIAN AND DEVONIAN PLATYCRINIDÆ.

The monocyclic base, the simple and apparently symmetrical cup, the slender pinnulate arms, and the lofty tegmen prevent us from seeking the relatives of this crinoid among the Inadunata (whether Larviformia or Fistulata, Monocyclica or Dicyclica), the Flexibilia of Von Zittel (=Articulata, *pars*, Wachsmuth and Springer), or those Camerata in which the lower brachials and interbrachials form an important part of the dorsal cup (e.g. Reteocrinidæ, Actinocrinidæ). Limiting our survey to the older Palæozoic crinoids, we see left only a group of genera, which Von Zittel, in his "Grundzüge der Palæontologie," 1895, refers to the Larviformia and the Platycrinidæ, which Wachsmuth and Springer include in the Platycrinidæ, but which Jaekel, in his fine "Beiträge zur Kenntniss der Palæozoischen Crinoiden Deutschlands" (*Pal. Abhandl.*, viii, Heft 1; Jena, 1895), distributes among both his Order Cladocrinoidea and his Costata, a Suborder of his Pentacrinoidea.

Among the Silurian and Devonian representatives of this group are *Cordylocrinus*, *Culicocrinus*, *Coccocrinus*, *Hapalocrinus*, *Agriocrinus*, and *Thallocrinus*. These all agree in having a dorsal cup composed of a pentagonal monocyclic base of three basals, one small and two large, and a circlet of five radials unbroken by any anal plate. The two primibrachs, though joined to the calyx wall by interradially situated plates, stand out clearly from those plates, and likewise



A new Silurian Crinoid from Melbourne, Victoria.

remain distinct from the radials. From the primaxil onward the arms are free, and occasionally one or both of the arm-branches fork again; pinnules are borne on each or on every other brachial; the IIBr (and IIIBr when present) may be uniserial, in zigzag, biserial, or fused in pairs. The interradially situated plates above mentioned belong rather to the tegmen than to the dorsal cup, the proximal ones only being to a certain extent interbrachial. The anus penetrates one of the interradial areas without materially affecting the symmetry of the calyx, and there is no anal tube.

This set of characters might be thought to define a fairly homogeneous group; and yet the relationships of individual genera to other genera of very different families, and even orders, have to some writers appeared so close that there has been great diversity in previous attempts at classification. For instance, the type of *Coccocrinus* was originally described by C. F. Roemer ("Rhein. Uebergangsgeb.," p. 63, pl. iii, fig. 3, 1844) as *Platycrinus rosaceus*; but Joh. Müller, in erecting the genus, considered that it was most closely allied to *Haplocrinus*.[1] D'Orbigny had already placed it, along with the *Cyathocrinus inæquidactylus* of McCoy, in a genus *Amblacrinus* next to *Cyathocrinus* ("Prodrome de Pal.," i, pp. 104, 156, and "Cours E'lémentaire," i. p. 145: Paris, 1849). C. F. Roemer doubtfully accepted *Coccocrinus*, as well as the position assigned to it by Müller, in "Lethæa Geognostica," 2nd edition, i, p. 229, 1855. F. J. Pictet also placed *Coccocrinus* in a family Haplocrinidæ ("Traité de Paléont.," ed. 2, iv, p. 310, 1857), and yet, referring to *Amblacrinus* (op. cit., p. 332), he says of the same species: "je n'ai pas pu comprendre en quoi ce diffère des vrais Platycrinus." Zittel's "Handbuch der Paläontologie" associates *Coccocrinus* with *Haplocrinus*; while P. H. Carpenter[2] and Wachsmuth & Springer have assigned it to the Platycrinidæ. Despite the arguments of these writers, Von Zittel, in his "Grundzüge," erects a family of Larviformia—the Coccocrinidæ—including *Coccocrinus* and *Allegacrinus*. Finally, Dr. Otto Jaekel, while recognizing the affinities of *Coccocrinus* to the Inadunata, especially *Hoplocrinus* (including *Bærocrinus*), places the genus in his family Hapalocrinidæ, far removed from the Platycrinidæ, in his suborder Costata. On the other hand, Wachsmuth and Springer would doubtless have included the Hapalocrinidæ in the Platycrinidæ, perhaps even in the single genus *Cordylocrinus*, to which they do actually refer the well-known Dudley species, *Platycrinus retiarius*, Phillips, which Jaekel puts in his new genus *Thallocrinus*.

While not denying a possible connection between the Hapalocrinidæ and the Plicatocrinidæ, and while recognizing the close affinity of the proposed suborder to the Inadunata Monocyclica, I must confess that I cannot find in Dr. Jaekel's detailed and suggestive description any grounds for severing *Coccocrinus*, *Thallocrinus*, and the rest from *Cordylocrinus* and *Culicocrinus*. The

[1] Müller in Zeiler und Wirtgen, "Bemerk. ueber d. Petrefacten, etc.": Verh. naturhist. Ver. preuss. Rheinl., Jahrg. xii, p. 21. (Bonn, 1855.)

[2] In Nicholson and Lydekker, "Manual of Palæontology," i, 428, 1889.

difficulties of the systematists seem to have arisen from the fact that the whole assemblage is intermediate between the older representatives of Inadunata Monocyclica and the Carboniferous forms of Platycrinidæ. In the "Crinoidea of Gotland," i, p. 19,[1] I suggested that the Camerata might have been derived from the Inadunata, and that such genera as *Platycrinus* might "represent stages in the evolution not far removed from the true Inadunate type." Dr. Jaekel's criticism of this sentence is based partly on the erroneous inference that I regard *Platycrinus* "als den Ausgangspunkt der Entwickelung der Cladocrinoideen (*Camerata*)" (op. cit., p. 38), and partly on his own supposition that *Platycrinus* is descended from *Marsupiocrinus* because the latter is older in geological time. It was only by getting *Coccocrinus* and "*Platycrinus*" *retiarius* conveniently out of the way, and by overlooking *Cordylocrinus*, that Dr. Jaekel could have written thus. All that is known of the ontogeny and phylogeny of crinoids favours the view that these and the allied genera mentioned above were derived from genera with a cup composed solely of 5 BB and 5 RR, with dichotomous arms clearly distinct from the cup, with the greater part of the tegmen covered by five large plates, between which the ambulacra passed to the mouth, and with very slight disturbance of symmetry due to the anus. On the other hand, *Cordylocrinus*, *Coccocrinus*, and their allies gave rise, in their turn, to the more highly specialized forms, first *Marsupiocrinus*, and at a later date *Platycrinus* and *Eucladocrinus*.

Revision of *Cordylocrinus* and the "Hapalocrinidæ."

Before considering to which genus the Melbourne specimen is most nearly related, some revision is required.

Wachsmuth and Springer ("Monogr. N. Amer. Camerata," ii, p. 735, 1897) have placed "*Platycrinus*" *retiarius* in *Cordylocrinus*. But according to Angelin's "Iconogr. Crin. Suec.," pl. xxiii, fig. 6, *C. comtus*, the type-species, bears two pinnules on each IIBr. The brachials are therefore compound, derived by fusion from biserial or zigzag pairs. The same appears from Hall's figures ("Palæont. N.Y.," iii, pl. iv) to be the case with the American species which Wachsmuth and Springer refer to this genus. Certainly the same arm-structure obtains in the undescribed Dudley species with two arms to the ray, mentioned by those authors on p. 736. In "*Platycrinus*" *retiarius*, however, the IIBr are uniserial, approaching the zigzag arrangement, and each bears one pinnule. This is surely enough for generic distinction.

Dr. Jaekel calls the last-mentioned species *Thallocrinus retiarius*, so that we must now consider the claims of the genus *Thallocrinus* to recognition. Jaekel's family Hapalocrinidæ is thus defined (op. cit., p. 94):—"Dorsal cup consists of a circlet of large spade-shaped radials, and a tripartite (2 + 1) occasionally fused basal circlet; tegmen essentially composed of five large orals, but sometimes there are distinct suborals [interbrachials]. The five arms fork

[1] K. Svenska Vetenskaps-Akad. Handl., Bd. xxv, No. 2. (Stockholm, 1893.)

above the second brachial into two main branches, which occasionally branch again. Each of these branches bears alternately disposed ramuli ["pinnules" of most authors], with very elongate ossicles. The columnals at some distance from the cup are very long, with a thickened ring in the middle, and often provided with cirri."

The family includes the genera *Hapalocrinus, Agriocrinus, Thallocrinus,* and *Coccocrinus,* of which the first three are new and based upon species from the Hunsrückschiefer of Bundenbach. The type-specimens, unfortunately, like most of the Bundenbach specimens at which Dr. Jaekel has worked with such zeal and intelligence, are very difficult of interpretation. The characters selected by the author of the genera as diagnostic may, so far as they are known, be set out in the following table:—

	Hapalocrinus	*Agriocrinus*	*Thallocrinus*	*Coccocrinus.*
BB	3	?	3	3
Small B	r. anterior	?	l. anterior (?)	l. ant. apud W. and Sp. Position varies apud Jaekel
RR	with axial folds	axial folds in some spp.	smooth	smooth
Orals	closely joined to suborals	?	?	5 large
"Suborals"	4 in post. IR	?	fused exc. in post IR	single
Anus	in oral	?	?	between oral and suboral
Arms	fork once	fork once; sometimes twice	fork once in type-sp. [twice in *T. retiarius* sometimes]	fork once; beyond which is unknown
Pinnules	regularly alternate; one on each IIBr, or on every other one [text and plate not consistent]	regularly alternate; one on each IIBr, or on every other one	regularly alternate; one on each IIBr	?
Columnals	long; with swollen ring in middle	short; with median thickening	short; with median thickening	short; ?
Cirri	radical only	radical, and occasionally on stem	no cirri in type-sp. (?) [all along stem in *T. retiarius*]	?

Two things are made clear by this table. First, that *Thallocrinus retiarius* does not agree with the type-species or with the original diagnosis of the genus in two supposed diagnostic characters. Second, that the differences between these genera are very slight and based

on very variable characters. The case is even less hopeful when we consider other species than the type-species, such as *Agriocrinus inermis*, which the author himself admits to be intermediate between *Hapalocrinus* and *Agriocrinus*, but which in some respects is even more like *Thallocrinus*. *Coccocrinus* seems to be distinct, in its large orals, single suborals or interradials, and the position of its anus. We know, it is true, very little about these features in the other forms, but what we do know suggests a somewhat different structure, and it would be well to keep them apart from *Coccocrinus*. But to split them up into three genera, is surely inadmissible on the evidence at present available.

I propose, therefore, to merge all these in one genus; for this we are bound to take the name of the type-genus of Jaekel's suggested family, viz. *Hapalocrinus*; and the type-species will be *H. elegans*, of which Jaekel has given so complete a description. The family Hapalocrinidæ lapses, since the genus falls into the Platycrinidæ as defined by Wachsmuth and Springer ("Monogr. N. Amer. Camerata," ii, 642, 1897). Their diagnosis of that family runs thus:—

"Monocyclic. Brachials and interbrachials only slightly represented in the dorsal cup; the lower brachials taking more or less the form of free arm-plates; the lower interradials in part interambulacral in position, rarely entirely interbrachial. Radials in contact all around, there being no special anal plate. Basals forming a pentagon." To this must be added: Two pairs of the primitive 5 BB fused, the small unfused B being usually the left, sometimes the right, anterolateral.

The genus *Hapalocrinus*[1] may be diagnosed thus: — Unfused B left or right anterolateral. Orals not large or distinct. Interbrachials (suborals of Jaekel) more than one (at least in post. IR). Ambulacrals (covering plates) visible between orals and interbrachials. IBr 2 (? sometimes 3). Arms fork once, sometimes twice, varying in this respect in the same species (or even individual). IIBr (and IIIBr when present) uniserial, or slightly in zigzag, bearing alternately disposed pinnules, either on each or on every other brachial, with pinnulars longer than wide. Stem circular in section, with small axial canal, relatively long and slender; the columnals, especially the nodals, with a median thickening. Cirri are present at the root, and often on the nodals of the rest of the stem.

To these characters it may be added that some species (e.g., *H. elegans*, *H. Frechi*) show axial folds passing from the radial facets to the basals; and that some species (e.g., *H. elegans*, *H. Frechi*, *H. retiarius*) have an ornament of slight longitudinal grooves on the brachials.

Coccocrinus differs from *Hapalocrinus* in its large, symmetrical orals, which almost cover the ambulacra, its single interbrachials, and (?) the position of the anus. *Culicocrinus*, which closely resembles

[1] ἁπαλός, 'tender,' like roast sucking-pig or the cheeks of a maid, also delicate in health. The extreme appropriateness of the name must excuse its close resemblance to *Haplocrinus* of the same geological age, as well as to *Hoplocrinus*, with which it is associated by its author.

Coccocrinus in the calyx-structure, further differs from *Hapalocrinus* in its heavy, biserial arms. *Cordylocrinus* is very closely allied to *Hapalocrinus*, but differs in the compound nature of the IIBr, each of which bears two pinnules. *Marsupiocrinus* has but one IBr, biserial arms, and a large five-lobed axial canal in the stem. *Platycrinus* and *Eucladocrinus* have but one IBr, biserial arms, and stem of elliptic section.

In its arm-structure, then, *Hapalocrinus* is more primitive than any of the Platycrinidæ, except perhaps *Coccocrinus*, of which the arms are unknown; in its tegminal development it seems to be about on the same level as *Coccocrinus*, though perhaps a little further removed from the Larviform type. This stage of evolution consists with its geological range, which is from Middle Silurian to Lower Devonian.

Reference of the Melbourne Crinoid to Hapalocrinus.

In its arm-structure, and, obscure though it be, in the composition of the tegmen, the Melbourne crinoid appears more closely allied to *Hapalocrinus* than to any other genus with which I am acquainted. It has, however, three IBr, whereas no other Platycrinid has more than two. Therefore, if it be placed with *Hapalocrinus*, we must regard it as in this respect more primitive than the other species. It is also at least as primitive as any other *Hapalocrinus* in the structure of the IIBr and pinnules. It would be unwise to found a new genus for the reception of this unique and rather obscure specimen, so I refer it provisionally to *Hapalocrinus*.

As there can be no question that it represents a distinct species, it requires a trivial name. I received the specimen and studied it during the week of Her Majesty's Diamond Jubilee; this fact, and the fact that it is the first Silurian crinoid crown found in the colony of Victoria, inevitably suggest the name—

Hapalocrinus Victoriæ.

Diagnosis.—IBr 3; arms fork once; IIBr half as long again as wide, in zigzag, bearing alternately disposed delicate pinnules.

The type-specimen will be placed in the museum at Melbourne University by Mr. T. S. Hall, to whom, in conclusion, I express hearty thanks for giving me the opportunity of studying this highly interesting fossil.

II.—Notes on the Age and Structure of the Gneissose-Granite of the Himalayas with reference to Mr. Middlemiss's Memoir on the Geology of Hazara.

By Lieut.-General C. A. McMahon, V.P.G.S.

PART II.

Origin of the Foliation.

In the first part of this paper I confined myself to the consideration of the age of the Himalayan granite; in this I propose to discuss Mr. Middlemiss's views regarding the cause of its foliation.

Mr. Middlemiss appears to be so satisfied in his own mind that the foliation has been produced subsequent to the consolidation

of the rock by pressure-metamorphism, that he considers it unnecessary to discuss the facts and arguments that I have from time to time brought forward to show that pressure *before* consolidation was the efficient cause of the foliation of the Himalayan rock. I shall, as far as possible, refrain from repeating in this paper what I have previously written on this subject, and shall content myself with referring the reader to my address, as President of the Geologists Association (Proc. Geol. Assoc., xiv, p. 287), for a condensed presentation of my case. On the present occasion I shall confine my remarks to a statement of supplemental facts and arguments.

Mr. Middlemiss's hypothesis, given in his own words, is as follows:—"Just as the mineral composition of the great crystalline axis of the Himalaya has been found to be the same right through from end to end of the chain, so is it also found to be the case that the dynamic metamorphism of those rocks is also a constant factor throughout the range.

"I do not think there can be two opinions as to the origin of this state of things. It can only imply a steadily acting lateral pressure of the earth's crust tending to bank it up against the central crystalline core by a movement and a resistance in two opposite directions. To the south we have the newer zones of disturbance showing no dynamic metamorphism; but to the north the accumulated pressures consequent on the upheaval of several zones one after the other, with the gradually diminishing in size of the larger earth-folds as the centre of the solid crystalline core is approached, have all tended to bring about that intense cleaving, shearing, and deformation of the minuter particles of the rock which I embrace generally under the head of dynamic metamorphism" (p. 279).

The author tells us (p. 61) that the gneissose-granite "is present in the form of intrusive bands among the schistose rocks"; that these "bands are generally parallel to the foliation of the rocks among which they lie"; and that the foliation of the granite is "perfectly parallel to that of the schists" (p. 66). Some of the granite is foliated, but some is not (p. 64). The non-foliated variety represents the "original condition in which the rock first solidified after intrusion among the sedimentary rocks of the region" (p. 64); and the foliated variety has been derived from it "by the mechanical action of pressure metamorphism" (p. 64); "acting on the rock since the time it became a solid stationary mass" (p. 65).

The evidence on which the author appears to rely for proof of his position may be summarized as follows:—

(1) The crushed condition of the Himalayan rocks generally (p. 279).

(2) The parallel foliation and fissility of the rocks (p. 279).

(3) The fact that the foliation of the gneissose-granite is usually parallel to that of the schists, although there are exceptions to this rule (p. 66).

(4) That the gneissose-granite exhibits augen structure (p. 65).

(5) That the minerals have been rolled and dragged out (p. 65), and exhibit "flaser" structure (p. 72).

(6) That the pebbles in the Infra-Trias conglomerate have been flattened and deformed (p. 55).

(7) That coarse grains of quartz in the Infra-Trias sandstone have been powdered and drawn out to eyes (p. 56).

I may say at once that I fully admit, and for the matter of that always have admitted, the truth of the facts alleged in the above seven points. What I have always denied, and still deny, is, that the foliation of the granite was produced by crushing and shearing *after* it became a solid mass. I have all along contended that a foliated structure was impressed on the granite *before* its final consolidation.

No one, I presume, doubts that the birth of the Himalayas was accompanied by great earth movements, and that during the elevation of these lofty mountains great compression, contortion, faulting, and shearing of the strata took place on a grand scale. I certainly have never denied it; and I have expressly admitted that the gneissose-granite itself bears evidence of having suffered many things from the dynamic forces of Nature. For instance, I stated in my Presidential Address already referred to, that "the granite was forced in thick sheets between beds of strata, and into the faults and fissures opened up in the course of the compression, uplifting, and contortion of the rocks which set in within the Himalayan area at the close of the Eocene period" (loc. cit., p. 288). I pointed out that in one region of special strain "the gneissose-granite passes at its edges into a rock that might, without microscopical examination, be taken for a mica-schist" (loc. cit., p. 288). In another paper (GEOLOGICAL MAGAZINE, 1887, p. 214) I said that the outer band of gneissose-granite at Dalhousie "not unfrequently looks, viewed macroscopically, as if it had been flattened under a steam-roller"; and on the same page I detailed the evidence of strain, fracture, and traction, supplied by the microscope.

The point at issue, therefore, is not whether the Himalayan rocks generally, or the granite in particular, give evidence of having suffered compression, contortion, or fracture, but whether the foliation of the granite was produced prior to, or posterior to, the consolidation of the rock. The production of evidence to prove points not at issue is illogical. When no one denies that the rocks of the Himalayas have suffered contortion and crushing, what is gained by proving what is already admitted? Moreover, as the question raised concerns an igneous intrusive rock, evidence to show the crushing of sedimentary rocks is really irrelevant, unless it can also be shown that the crushing relied on took place *after* the entrance of the intruder.

The first and main point at issue in the case of the granite of the Himalayas, as before said, is whether the foliation was superinduced *before* or *after* the complete cooling and consolidation of the rock. Evidence to be of logical value must be directed to that issue.

From first to last I have contended that the foliation of the Himalayan granite was produced before the consolidation of the rock, and in my Presidential Address before referred to I tried to explain briefly that lateral pressure applied to an intruded igneous

rock in which crystallization had commenced prior to its being moved into place, would, if the pressure were kept up during the subsequent stages of its cooling, whilst it was "gradually progressing towards rigidity," inevitably result in the production of foliation, and would account for the "undulose extinction, fractured minerals, and crumpled micas, and such like evidence of strain and mechanical action," to be found in the Himalayan gneissose-granite (Pres. Address, loc. cit., pp. 288–9). Given tangential pressure and strain exerted on an intruded granite during its gradual cooling, these results must follow.

The above explanation, at all events, offers a definite theory to explain the foliation and other phenomena to be observed in the Himalayan granite, and it is no answer to this theory to point to evidence of strain and mechanical action in the rock; for the existence of strain and mechanical action during the critical period in the history of the granite, is an essential part of the theory itself.

I have looked in vain through Mr. Middlemiss's Memoir for evidence, other than the above, to rebut my theory. My second Presidential Address, dealing with the structural characters of the granite, probably did not reach the author until after his Memoir was set up in type, but my address was, in the main, a condensed statement of a theory long held by me, and to be found in my previous papers. The one published in this Magazine in May, 1887, alluded to above, for instance, contains an abstract statement of the theory in question.

It may be as well to remark here that so far as the *foliation* of the Himalayan granite is concerned, the foliation set up was a change of structure only, and did not involve any metasomatic change in the chemical composition of the minerals. The foliation was, in its main features, a parallelism in the orientation of the minerals which already existed in the still plastic magma, and was superinduced by pressure applied before the final consolidation of the rock. The mineral principally concerned in producing the foliation was mica, an original mineral abundantly present in the granite before foliation set in.

Given an abundance of mica in a still plastic granite, I do not see how great pressure could be applied without causing the platy leaves of mica to arrange themselves with their flat basal faces at right angles to the direction of the pressure. We have here, then, a *vera causa* to account for the foliation of an igneous rock. Given a sufficient amount of mica, or other mineral of similar habit, and sufficient pressure applied between the formation of the mica and the consolidation of the rock, and we have a sufficient explanation of the foliation it may exhibit.

It may be as well to point out that the tendency of platy minerals to arrange themselves under pressure with their flat faces at right angles to the direction of the pressure, was probably aided by other contributory causes. Even after the granite was moved into place, and the crystallization of some of its minerals, begun in plutonic

regions and continued after its intrusion into the sedimentary rocks, had progressed considerably, the still uncrystallized residuum, or mother liquor, must have been, to some limited extent, capable of flow or motion. Tangential pressure caused by earth movements, continuously acting on the granite, would probably force some of it into new fissures and newly-created planes of weakness, along the margins of the intrusive mass; and this marginal extension of the intruded granite, combined with a continuance of the tangential pressure, would create a slight flow in the unconsolidated mother liquor, sufficient to allow the mica and other platy minerals to yield to the pressure and orient themselves with their flat faces at right angles to it.

Further, it seems probable that the pressure exercised on the granite by the molten mass below it fluctuated from time to time. Deep-seated movements, such as laccolitic intrusions in neighbouring areas, or the extrusion of acid lavas at the surface, would, for a time, relieve this pressure and cause a small movement in or back flow of the mother liquor. In other words, fluctuations in the resistance offered by the molten mass under the granite would allow the partially consolidated granite to be compressed by the tangential pressure continuously exercised upon it.

A very small amount of interstitial movement would suffice to cause the flat leaves of mica to be arranged at right angles to the pressure, while a back flow to any extent, by depriving the crystallized minerals of the elastic support of the mother liquor, would lead to their crushing and deformation.

Those who have watched the operations conducted in a sugar refinery will readily understand that the mother liquor may be drained out of a mass of crystals without the crystals being carried away with it. I do not wish to suggest that the mother liquor was ever pumped out of the granite to any great extent: I only desire to make it clear that small interstitial movements might result from the causes above explained; and that even small movements would suffice to give the tangential pressure that was compressing the partially consolidated granite a free hand, so to speak, and permit it to impress a foliated structure upon the granite.

Interstitial motion created in the two ways suggested above—namely, by the marginal extension of the granite, and by a temporary diminution of the resistance offered by the molten mass below it—might be very local in its action, and hence some parts of a granite might become foliated while other parts remained unfoliated.

One more point remains to be mentioned. The earth movements that produce faults often persist for some time, and though a rupture in the folded strata gives temporary relief to the strain, the stress is sooner or later repeated, with the result that either the throw of the fault gradually increases, or the walls of the fault are moved up and down at intervals. Movements of the walls of a fault into which an igneous rock had been intruded would, as the solidification of the intruder approached completion, inevitably produce marginal foliation and pronounced crush-structures.

In the above remarks my chief object has been to bring before the reader, as clearly as I could, what I believe to be the main factor in the production of the foliation observable in the Himalayan granite. Other causes contributed to produce the effects observed. Traction when the rock was being moved into place,[1] and the corrosive action of the acid residuum on the phenocrysts, might be named; but it is needless to dilate on them here, as I have already written much in other papers on these themes. I am glad to see that Mr. Middlemiss recognizes the validity of the last-mentioned factor, for he remarks (p. 63) with reference to the porphyritic crystals: "The larger ones have often only a very vague outline in the rock, as if their boundaries had become indistinct from the reaction of the still molten magma upon them whilst the rock was in a half-consolidated state."

It is a great relief to be able to turn at this point in the discussion to an interesting and important piece of work in the Scotch Highlands by the officers of H.M.'s Geological Survey. I allude to the explanation of Sheet 5, Memoirs of the Geological Survey, Scotland, by Messrs. Horne, Peach, and Teall, 1896. In describing the granite of the south-east portion of the county of Kirkcudbright, Mr. Horne tells us at p. 23 that "the granite occurs mainly in the massive form throughout this extensive area, but there are certain limited tracts where it has a marked foliated character. The latter variety is a marginal phenomenon. It does not, however, extend continuously round the border of the igneous mass, being restricted to well-defined areas. . . . . As the observer passes inwards towards the centre, he finds the foliated variety of the border tract gradually to merge into the massive granite."

The granite contains basic inclusions and acid veins, which are also foliated, the foliation of the granite, basal inclusions, and acid veins having a common direction. "The evidence obtained in the field," Mr. Horne writes (p. 24), "clearly points to the conclusion that the foliation must have been superinduced on the massive granite with the basic inclusions, after the formation of the acid veins."

I infer, from what follows, that the acid veins must have been formed after the granite was moved into place, but before it cooled down. The foliation of the basic inclusions was evidently due to the cause that produced the foliation of the granite itself; and as the latter is attributable, as will be seen later, to pressure before cooling, I infer that the basic inclusions were made plastic by the contact action of the granite before the pressure was applied.

"After the development of the foliation," Mr. Horne continues (p. 25), "the foliated granite was injected by a well-marked series of dykes. . . . . These dykes run in a north-westerly direction, and have fine-grained or chilled edges. No single instance has been observed among them of any trace of foliation."

[1] Sir A. Geikie mentions interesting cases of "extreme mechanical deformation by the movement of a still molten rock" in his paper on the Tertiary Basalt Plateaux of N.W. Europe (Q.J.G.S., vol. lii, p. 333).

In order to realize the full significance of the presence of unfoliated dykes in the foliated granite, it is necessary to bear in mind that the dykes appear to have been derived from the same source of supply as the granite itself. Mr. Teall tells us at p. 45 :—"It follows from the above description, that there is very little difference in composition between the dykes of this type and the granitic rocks of the Criffel and Dalbeattie mass. . . . . It is probable, therefore, that the structural differences between the porphyrites and the hornblende granites or quartz diorites are due to differences in the conditions under which the magmas consolidated. If we suppose that an intrusion of the granitic magma took place in dyke form after the greater part of the hornblende, biotite, and plagioclase had separated out, we shall have a satisfactory explanation of the difference in structure between the porphyrites and the granitic rock. In applying this theory, however, it must be remembered that dykes of the type now under consideration occur not only in the sedimentary rocks, but also in the granitic masses. Moreover, dykes are found in the foliated area unaffected by the foliation. Such dykes cannot be regarded as in any way representing apophyses of the granite. They belong to a later phase of igneous activity. The source of supply was probably the same as that from which the main igneous mass was derived; but if so, it must have been tapped at a time when crystal building had proceeded to a considerable extent."

It would appear, therefore, that although the foliation of the granite was affected after the formation of the acid veins, it was completed before the porphyrite dykes were intruded into the granite; and as the source of supply of the dykes and of the granite was probably the same, no great lapse, measured by geological time, is likely to have intervened between the foliation of the granite and the intrusion of the unfoliated porphyrite.

Mr. Teall, at p. 43, remarks :—"The interstitial movement which has resulted in the production of foliation, and which is proved by the field evidence to have been produced by earth-stresses operating after the rock had reached its present position, and after the formation of the acid veins, has not affected all the constituents alike. The quartz and alkali-felspar which, as we have already seen, were the last constituents to solidify, are those which have yielded most to the deforming stresses. They show signs of crushing, and frequently pass into thin micro-crystalline or even crypto-crystalline streaks, which wind round the comparatively uncrushed crystals of oligoclase. The distribution of the ferro-magnesian constituents has been modified by the movement. Instead of being irregularly distributed through the rock they tend to form flaser in association with the micro-crystalline aggregate of quartz and alkali-felspar. The oligoclase has been little, if at all, affected by the movement. These facts clearly prove that the minerals which yield most readily to the deforming forces are those which consolidate last. *It is probable that the pressure acted before the rock-mass had actually cooled.*" The italics are mine.

The above description of the petrographic characters of the Scotch rock might be applied word for word to the Himalayan granite. It is gratifying to be able to point to the independent evidence of an expert who shows that it is "highly probable" that the pressure which produced the foliation of a Scotch granite "acted before the rock-mass had actually cooled." It further appears from Mr. Teall's remarks that the granite was in a partially consolidated state when the pressure was applied. "The last constituents to solidify," he writes, "are those which have yielded most to the deforming stresses"; and the pressure applied to the uncooled and partially solidified rock crushed the quartz and felspar crystals to such an extent that they passed "into micro-crystalline and crypto-crystalline streaks." We get "flaser" structure, and, in the words of Mr. Horne (p. 25), "The quartz and alkali-felspar are the constituents which yielded most readily to dynamic agencies. The yielding of these constituents dragged out the biotite and hornblende." The correspondence of the Scotch and Himalayan rocks in the origin of their foliation, and in their crush and flaser structures, would seem to be complete; and the testimony of Messrs. Horne and Teall to the effect that these structures, in the case of the Scotch granite described in the above Memoir, were probably produced whilst it was still uncooled and incompletely solidified, is all the more important as these observers have been by no means unfavourable to the cause of dynamo-metamorphism.

If, then, we can get "flaser" structure, and the crushing and dragging out of minerals into streaks by pressure applied to an igneous rock prior to its complete cooling and solidification, it seems to me simply nonsense to appeal to flaser structure, crushing, and the dragging out of minerals, as conclusive evidence that the rock exhibiting these structures has been deformed *after* its consolidation. Those who wish to establish that conclusion for any particular igneous rock must produce good and convincing evidence to prove that the dynamic movements took effect *after*, and not *before*, its consolidation.

The condition of mind of those who think that it is only necessary to produce evidence of crushing to close controversy on the point at issue, reminds me of those who, in days gone by, exclaimed with enthusiasm as they stood gazing at a gabbro: "We know for certain that this is a metamorphosed sedimentary rock, *because* it is foliated!" Lest any should think this statement exaggerated I will give extracts from a recently published memoir (60th Report, U.S. Geol. Surv. 1894–5), "On the Principles of North American Pre-Cambrian Geology," by Mr. C. R. Van Hise. "A few years ago," he writes, "it was assumed as a matter of course that distinct lamination in the gneissose granites, gneisses, and schists, and the fact that these rocks were believed to grade into the clastics, were taken as conclusive evidence of the original sedimentary origin of the whole" (p. 748); and the author refers by name to five authors who, among others, actually acted on the above-mentioned principle. Van Hise also points out (p. 768)

cases in which "laminated gabbro" was, in deference to this principle, written down as a sedimentary rock![1] I trust we have now nearly heard the last of arguments which may fairly be paraphrased into such a remark as—"This is a striking case of dynamo-metamorphism. It is quite evident that the rock was deformed by tremendous earth movements *after* its solidification, *because* it shows flaser structure and crushing, and some of its minerals have been drawn out into strings."

Turning again to Mr. Middlemiss's Memoir, there are some facts in it which, to my mind, raise difficulties in the way of the acceptance of the theory advocated by the author, and suggest the existence of evidence against it. Mr. Middlemiss tells us at page 75 of basic dykes which generally "form narrow bands or dykes roughly parallel to the foliation of the intruded rock"; but "in certain places the dykes cut across both the schistose series and the gneissose-granite intrusions, and also send off small veins into them." With the exception of the Indus Valley dykes, nothing is said about the foliation of the basic dykes that cut across the foliation of the schists. Are they foliated or unfoliated, and if the former, is their foliation parallel or at an angle to that of the schists? In fig. 4 (p. 76) one of the Indus Valley dykes is represented, and parallel fine white lines are to be observed along one edge of the dyke which appear intended to represent foliation. If these lines are intentional, and not an accident in printing, it is to be observed that they are at a high angle to the foliation of the schists and only occur along one margin of the dyke.

The author tells us (p. 75) that the dykes which cut through the gneissose-granite of the Indus are foliated along their edges. No information is given as to the direction of this marginal foliation in relation to the foliation of the granite; but it seems clear that the conditions under which the foliation was impressed upon the two rocks must have been different. If the foliation were imparted to the granite after its solidification, and after the intrusion of the dolerite, it is difficult to understand why the dolerite was not foliated from side to side.

In the detailed account of the Indus Valley section given in the Memoir (p. 253) a mass of dolerite is shown in fig. 51, as an intrusion in the schists parallel to their bedding. The section suggests the idea that the dolerite was folded up with the strata, and that the latter form a somewhat crushed anticlinal. The two dolerite bands on the right of the illustration might even represent the dying out of the dolerite seen on the left of the woodcut. The schists and the granite seen in this section appear from the author's general remarks (see ante, p. 346) to be foliated in a direction parallel to the folds of the bedding; but the dolerite, which was apparently folded up with the schists and the granite, and must have participated in the stress, crush, and shearing, which accompanied the folding of the rocks, is absolutely without foliation. If

[1] "This gabbro," he writes, "was formerly regarded as a later sedimentary series!" (Loc. cit., p. 768.)

the tangential pressure which crumpled up the strata produced foliation in the schists and granite, how did the dolerite escape uninjured? This is the more remarkable as dolerite takes on foliation very readily. The author has not given any explanation of the difficulty.

Mr. Middlemiss appears to rely very much on shearing and molecular movement to account for the results which he attributes to dynamo-metamorphism. "If we believe," the author writes, "(as we have a right to do from considerations as to the deep-seated nature of granite) that very great thicknesses of the slate or schistose rocks once overspread that crystalline core, we can understand that lateral stresses, if sufficiently powerful and long-continued, acting on such a material would be unable to violently fold and plicate them on a large scale, but would spend themselves in minute deformation of the rocks in shearing and in other ways characteristic of dynamic metamorphism" (p. 280).

If shearing and molecular movement took place in Mr. Middlemiss's crystalline and metamorphic zone to the extent supposed by the author, great heat must have been generated, and strong chemical action must have resulted. It is curious, therefore, to notice that these much "mashed" rocks, to use a term employed by geologists over the Atlantic, do not appear to have yielded the author much in the way of dynamo-metamorphic minerals. One reads of mica and of "incipient garnets," "which occur as minute specks or raised blisters" (p. 55), but I cannot call to mind anything else. This poverty of chemical and mineralogical result after the expenditure of so much dynamic energy recalls an ancient proverb to the effect that "a great mountain was in labour and brought forth a small mouse." [1]

I see no reason to believe that the great masses of gneissose-granite now to be seen in the Himalayas were "mashed" by pressure after cooling and solidification. In following outcrops along the line of strike, as for instance along the Shankan outcrop in the Simla regions (Records G.S.I., x, p. 217), the granite passes repeatedly from massive to foliated, and from foliated to massive, in an apparently capricious manner. This feature I think quite incompatible with the supposition that the foliation is the result of shearing subsequent to the solidification of the rock, either in the direction of the strike or in the direction of the dip.

I would again invite attention to the heliogravure of an inclusion in the Dalhousie gneissose-granite published at p. 174, in vol. xvii, Records G.S.I. The plate represents a cross-section of a splinter of schist two feet long, shaped like a tent-peg. The outlines of the inclusion are perfectly sharp, and it is evident that the granite in the locality where the inclusion was found cannot have been sheared after the inclusion of the fragment. Had shearing taken place the inclusion would have been flattened and drawn out into ribbons.

To sum up, I think that though the facts brought before us in Mr. Middlemiss's Memoir are interesting, they shed no new light

[1] "*Parturiunt montes, nascetur ridiculus mus.*"—Horace.

on the origin of the foliation of the granite of the Himalayas, and do not in the least traverse the theory I have offered to explain that phenomenon. The evidence supplied in the Memoir under review is on points which are not only not in dispute, but actually form an essential part of my theory. Nothing more need be said by me unless a serious attempt be made to meet or disprove that theory.

III. — The Geologically Recent Origin of the Surface Contour of Scandinavia and Finland, and its Lessons.

By Sir Henry H. Howorth, K.C.I.E., M.P., F.R.S., F.G.S.

SOME time ago I discussed in this Magazine the recent geology of Greenland; and I ventured to argue, and, I think, to prove, that the general rise of that great island from the sea, which is conceded by every inquirer, has been unbroken, and that during the so-called Glacial period, instead of being a lofty plateau as it now is, it was a comparatively low-lying land. I ventured further to argue, and also, I think, to prove, that with the rise from the sea of this great island there has been a corresponding fall in its temperature, which is a very natural and simple *a priori* meteorological conclusion, and to adduce a number of biological facts to show that during the so-called Glacial period Greenland, instead of having been mantled with an ice-sheet as it is now, had a comparatively mild climate; and that its scanty fauna and flora, instead of having been the result of sporadic migrations thither since the so-called Glacial age, is, in fact, the expiring and shrinking remnant of a once much more important life. The facts and arguments I adduced have nowhere been traversed or met, and it seems to me they are unanswerable.

I now propose a more hazardous work, namely, to apply the same arguments I used in regard to Greenland to Scandinavia and its borders, more hazardous because Scandinavia is the very sanctum of the Glacial geologist, and in attacking him there we attack him in his citadel. I may premise my remarks by saying that I have paid two visits to Sweden, one to the island of Gothland, and one to Finland.

That Northern and Central Scandinavia have recently risen from the sea, has been known since the beginning of the last century, and has been the subject-matter of many memoirs and many researches, in which Lyell, among others, distinguished himself. I do not know anyone that disputes or denies the conclusion. The fact is thus stated by that picturesque and careful writer, Réclus:—Elevated beaches, which can be traced by the eye like the steps of an amphitheatre, are arranged in stages at various heights on the slopes of the mountains. Heaps of modern shells are found at heights of 500 to 650 feet above the level of the sea; and the great branches of pink coral formed by the *Lophohelia prolifera*, which lives in the sea at a depth varying from 1,000 to 2,000 feet, are now raised up to the base of the cliff.[1] The pine-woods, too, which clothe the

[1] Carl Vogt, *Nordfahrt*.

summits, are continually being upheaved towards the lower snow limit, and are gradually withering away in the cooler atmosphere; wide belts of forest are composed of nothing but dead trees, although some of them have stood for centuries.[1] ("The Earth," p. 623.) Although this movement of the Norwegian land is widely recognized, it does not seem to have occurred to the champions of the Glacial theory that it has been going on for a long time; and so far as every geological piece of evidence goes, it extends back to the so-called Glacial period. There is evidence that this rise has taken place to the extent of 600 feet in certain parts of Norway. If this be so, then it follows that Scandinavia is much higher at present than it was at the period in question; and if so, it as necessarily follows that, *cæteris paribus*, its climate is now more severe than it was at the period in question, for the general climatic conditions of a country are measured, other things being equal, by the amount of its elevation above the sea-level.

This is an elementary inference which seems to have escaped the champion of the Glacial theory, who, visiting Western Norway for the first time, or many times, and sailing or steaming in and out of its fjords, is struck, as every visitor is struck, by the smoothed and polished walls of rock he sees, by the rounded and mammillated appearance of the low islands he passes, and is disposed to break out into magnificent rhetoric on the subject. The facts are plain enough, their explanation is not so plain. To *the Glacialist* freshly arrived from England, or perhaps Scotland, full of icy enthusiasm, there could be no plainer proof of ice-action on a gigantic scale than these polished and smoothed walls and rounded bosses of crystalline rocks.

To the sceptic and the Philistine, the natural promptings of a heretical mind are to try and discover some other area where the same conditions occur, and where the story is more plainly written, and he naturally turns to Greenland, another rising land, where the polishing and rounding are of exactly the same kind; but, inasmuch as the movement of the land is more rapid, the story is clearer. In Greenland and in the archipelago of islands north of America, we have, as I say, precisely the same phenomenon to explain, but there it has been explained, by those who follow inductive methods, not as the consequence of an Ice Age, but as the result of recent submarine submergence and the continued beating and wearing and washing of the rocky surfaces of a rising land, assisted possibly in part by shore-ice and the movement of ice hummocks and gravel, etc. These are causes which would certainly produce the effects presented by Western Norway, and which have been attributed to a hypothetical ice-sheet. This is not all; these are the results which must inevitably have followed from the considerable submergence of the Scandinavian peninsula, which is not hypothetical at all, but which is one of the best established conclusions in geology.

This being so, I claim that the attribution of this polishing and

[1] Keilhau, Bull. Soc. Geol. France, 1st ser., vol. vii.

rounding of the surfaces of Western Norway, which have been so generally and widely accepted as proofs of the former existence of a great ice-sheet, is not well founded, and that this hypothetical ice-sheet has had to do with this particular phenomenon as little or as much as the west wind has. The fact is, that these polished and rounded surfaces, when they occur on the coasts of Norway, on the islands off its shores, and in the Lofoden Islands, and up to a certain point only, are the measure, not of the potency of a former ice-sheet, but of the amount of submergence to which the land has been subjected in quite recent geological times. I am not the first to make the observation, although I press it further than Pettersen did. That well-known Norwegian geologist, who probably knew this coast better than any other Scandinavian geologist, urged over and over again that certain mammillated surfaces and polished walls of rock in Norway are clearly and definitely the result of marine and tidal action, precisely similar to that going on wherever crystalline rocks are subjected to the washing of the sea, and nothing more. This is surely a very important and useful conclusion, and one which decapitates many exotic adjectives which have found their way into the literature of the Ice Age, and takes away one of the most potent props of that very ricketty cripple the North Sea ice-sheet, for it makes the Northern peninsula an even more inadequate nurseryland of great masses of ice.

It is perfectly plain that this rise of the Norwegian lands was shared by that of Sweden, Lapland, Finland, and Northern Russia. We have an immense quantity of evidence all pointing most emphatically to the rise having taken place all round the Northern Baltic seaboard since the so-called Glacial age, and evidence of it is forthcoming from all parts of Sweden and Finland in the widespread beds of stratified sands and clays which are found at various heights up to 1,500 or 2,000 feet, and in the shell-beds and other similar débris, and which show that not only has Scandinavia been rising for a long time, but also that it was not long ago in great part submerged, and formed part of a wide-spreading sea extending from the Dovrefelds to the Petchora.

Perhaps the most striking proof of this is the evidence of the fauna of the great lakes of Sweden and Northern Russia, *Relikten seen* as they have been called. They contain a number of species of animals clearly of marine types, curious biological witnesses to the fact of their having in recent geologic times formed part of a continuous sea. This must have been the very last chapter in their history, or these forms of life would have been exterminated.

Dr. Ludolf Credner has collected some very important evidence in regard to this matter in his well-known memoir on *Relikten seen*, published as one of the supplements to Petermann's "Mittheilungen" in 1887. He has shown that in thirteen Swedish lakes, namely, Wener, Wetter, Mölletjärnet near Krokfors, Leläugen, Stora-Lee-Sjön, Änimmen in Dalsland, Glassfiord, Fryken in Wermland,

Möckeln, the Mälar, Laxsjön, Barken, and Velangen near Upsala; in six Norwegian lakes situated to the east of the Dovrefelds, namely, Mjösen, Tyrifjord Af-see on the Dovrefeld, Sogusvandent and Eivvaugen near Christiania, Hurdals and Stor Sjön in Odalen; and twelve lakes in Northern Russia and Finland, namely, Ladoga, Onega, Höytiäinen, Pyhäselka, Rehja, Uleâtrask, Saimâ, Kalavesi north of Frederikshamm, Maaninga, Päljänne north of Helsingfors, Pielesjärvi north-west of Ladoga, and Nasijärvi—there are now living a number of marine forms which point most forcibly to all these lakes having once formed part of the ocean.

These consist of a seal (*Phoca annellata*), which occurs in several of the Finnish and Russian lakes, including Ladoga, Onega, and Saimâ; a number of Crustaceans, including *Mysis oculata, Pontoporeia affinis, Pallasca cancelloides, Gammaracanthus loricatus, Idotea entomon, Cythere lacustris,* and *Limnocalamus macrurus*; and three varieties of fish, namely, *Cothis quadricornis* and two forms of trout, which have become differentiated, apparently, from not now having access to salt water, namely, *Trutta Salar* var. *relicta*, and *Trutta lacustris* (op. cit., pp. 45–8). The greater part of these forms are Arctic ones, and point to the widespread mother sea of these various lakes having been connected with the Arctic Ocean. The lakes do not stand alone. The country in various directions is marked by beds containing Arctic marine shells, clearly proving a very recent submergence. In the north, says Credner, in Dalsland, Vermland, Dalecarlia, Gestrikland, and Helsingland, these beds exist to a height of 500 to 550 Swedish feet above the sea-level; in the south of Sweden they are not found so high; in Scania at 100 feet; in the neighbourhood of Lake Finga at 200 to 250 feet; near Braninye, near the southern point of Lake Wetter, at 700 or 800 feet; and he urges that up to this height Sweden has, since the Glacial period, been covered by the sea. The proofs do not merely consist in loose beds. Brongniart long ago described finding a surface of gneissic rocks at Uddevalla at a height of 200 feet above the sea-level, with barnacles (*balani*) adhering to the rocks, showing that the sea had remained there for a long time; and Lyell himself reports how in 1834 he found at Kured, about two miles north of Uddevalla, and at a height of more than 100 feet above the sea, a surface of gneiss newly laid open by the partial removal of a mass of shells, and so firmly did the barnacles adhere to the gneiss that he was able to break off portions of the gneiss with the shells attached. The face of the gneiss was also encrusted with Bryozoa. (Lyell's "Principles," ed. 1868, ii, 192.)

Another class of evidence is afforded by the so-called giants' cauldrons. For some reason or other these have been connected with glaciers and ice-sheets. I know not why. It is as plain as can be that they are the result of water in motion acting upon arrested stones, which are set spinning or turning round and thus erode deep holes; and wherever found they bear testimony to the more or less continued flow of water, and have nothing whatever to do with ice

or its handiwork. They have been described as occurring in various parts of Sweden, and from their position attest the great alterations of contour of the country since they were made.

Bergmann, in the last century, mentions six as occurring at Kokare Fiarden, in Finland. One of them was still under water of a foot deep, and was only thirty years old when he wrote, and its actual growth had been watched by Nordenskiöld; and he urges that others of a similar kind, out of reach of the present streams, had the same origin. He mentions others in Jemteland, in West Gothland, and in Bohuslan. Sefstrom also describes these giants' cauldrons, or Jättegrytor as the Swedes call them, and points out that they are made by our present rivers, such as those near Avesta; and tells us that he himself saw some uncommonly beautiful ones some years before, while they were digging the canal on the side of Ostra Dalefven. Near Gagnefs Grada several of these cauldrons were met with, some large enough for a person to stand in. They were situated where a river formerly flowed down a slope. Others are mentioned as existing on the side of a hill near Stockholm between Roslagstull and Albans, apparently quite out of the route of any now possible river or stream. A famous one, in which several people can sit, exists on the mountain above the present falls of Trollhältan, and was therefore made by water when the contour of the country was entirely different to what it is now.

Let us now turn to another kind of evidence. This was adduced by that very acute observer Durocher, and accepted by Murchison, namely, the composition of the sand which forms such a notable part of the surface beds of Scandinavia. Durocher observed and described large horizontal tracts of sand in which only the quartz-grains remain, the mica and felspar of the original matrix having been in a large measure dissolved and washed away, a condition of things only to be explained by aqueous causes, and quite different to the sands due to the disintegration of granite *in sitû* either by ice or other means. This submergence seems to me to be completely confirmed again by the shape and contour of the surface of these northern lands: the rounded, whale-backed or mammillated surfaces, polished and smoothed, the rolled and rounded boulders, the polished pebbles in the gravels—all these are precisely paralleled by the phenomena reported by the Arctic navigators as marking the surface of the North American Archipelago, and that part of the northern shores of the continent which we know has only just emerged from the sea, and it seems to me we must attribute them to the same cause.

That this movement is shared by Finland, is also well known; and there also we have the same kind of evidence, except that marine shells are very scarce; but as an old writer says: "It is impossible not to perceive that the top of every rock *in sitû*, every tor, every hill and knoll of granite, or primitive rock, from Carelia to the Gulf of Bothnia, presents a surface as distinctly rounded and water-worn as the boulders or colossal pebbles that lie around their bases."

Durocher described long ago the wide stretches of rudely stratified

sands which he met with in Finland, and which are only consistent with aqueous action. The most experienced and recent authorities in Finland, such as Nordquist and R. Hult, are of one mind about it. The former speaks of an Arctic sea which covered the greater part of Scandinavia, Finland, and Northern Russia (see Zool. Anzeiger, nn. 254–5); and the latter, in a recent geological memoir, collects the evidence for the recent submergence of Southern Finland, especially the country round Helsingfors; while Credner, whose prejudices are all the other way, is constrained to say: "*Indessen, so bestimmt aus Zahlreichen Anzeichen auf eine früher umfassendere Wasserbedeckung Finlands geschlossen werden muss.*" ("Relikten seen," part 2, p. 14.)

I would therefore urge that the recent history of the Scandinavian peninsula and Finland is the rise of the whole country from a condition in which a large portion of it was under the sea, until it became dry land as at present. This view has forced itself on everyone who has examined the country on the spot, and was held as much by Lyell and C. Martins as by Sefstrom and Durocher, by Credner and Erdmann. If so, it follows, it seems to me, that the climate of Scandinavia and its borders, like that of Greenland, instead of having become milder, has become more severe in these later times; and we can hardly doubt that when Scandinavia formed an archipelago of islands, constituted by those portions of it which are still unworn and unweathered and surrounded largely by water, those islands, instead of having been the nursery-ground of snow-sheets and icebergs or of glaciers much larger than those now existing, had a very different climate indeed, the land having been probably the home of trees, and the sea full of molluscs, etc. I cannot see how and where we are to find room or place between this condition of things and to-day for an Ice Age, with its portentous machinery. If there ever were an Ice Age in Scandinavia, therefore it was before this submergence, and not after it; and the majority of the champions of such a period, when the greater part of the country is supposed to have been blanketed in ice, put it in fact on the other side of this submergence. They claim further that before the land was covered by the sea it stood at a probably much higher level than it even does now, and was consequently capable of nursing a great ice-sheet. Of direct evidence of this former higher level of the land, I know none whatever. It is a postulate entirely created to meet the exigencies of the ice-men, and is completely arbitrary, and until evidence of it is forthcoming it ought to be erased from our books.

To Erdmann, the latest Swedish authority on the Ice Age, the beds which especially attest the action of ice in Sweden, and which he styles morainic, are not those formed of rounded pebbles, which he naturally assigns to the action of water, but the so-called angular gravel and the angular erratics. First let me remark that these beds of angular gravel have no resemblance whatever to moraines in their external appearance. They are not arranged in mounds, concentric or otherwise, but are laid down in continuous mantles, quite unlike

all glacier beds known to me, nor are their contents in the least like ice stones. Again, Durocher long ago showed that the greater part of the Drift of Scandinavia has not been derived from the Dovrefelds and the higher lands between Norway and Sweden, or from Lapland, but from the much lower grounds of Central Sweden. He says distinctly that the mineralogical structure of the Scandinavian blocks found in South Sweden and in North Germany shows that they came from the low and mammillated hills of Sweden and Finland, and not from the higher mountains. Those mountains are schistose, while the greater portion of the blocks are granitic, and granite occurs in those mountains in only very small quantities. (Bull. Soc. Géol. France, 1846, pp. 64–5.) He thus objects to the theory of Murchison and Verneuil that the erratics were derived from the lands projecting above the ancient Northern Sea, whose existence everyone admits, and that they were carried away from them by icebergs, and thus floated over the water. This latter view seems quite untenable in the presence of the fact attested by Durocher.

(*To be concluded in our next Number.*)

IV. — Low-tide Causeways.

By the Rev. Edwin Hill, F.G.S.

I SUPPOSE that most of us are familiar with the processes by which necks of land are cut down, by which promontories are turned into peninsulas, and the peninsulas into islands, while the islands in their turn become rocks. I have elsewhere called attention to all stages of the process as shown in the four corners of Sark. On the south there is the Coupee causeway, an isthmus 200 feet above sea-level, with a crest only some six feet in width; on the north, an isthmus yet lower, and parts beyond already being separated by the sea; on the west, an island, Breqhou; on the east, rocks, the Burons. There are to be seen all stages of the process, but does that process always proceed through all those stages to the end?

Many years ago, one in a party of frolicsome youths, I swam with them to an islet in a lonely Scotch loch. We intended to explore the unexplored, for there was no boat on the loch. We were, however, rather disappointed by the discovery that this apparently inaccessible islet was really joined to the land by a submerged ridge scarce under water; we need not have swum, for shallow wading would have sufficed. Since then I have frequently noticed such bars similarly joining rocks or islets to a neighbouring mainland; I have noticed them not only in such fresh-water lochs, but on the coast of the sea as well. Such, for instance, are the connections, passable at low tide, between the Jersey beach and Elizabeth Castle in St. Aubin's Bay; between Guernsey and Lihou, formerly also the access to Castle Cornet; there are many others, less conspicuous, in the Channel Islands. For all these, I think there is the same natural origin.

The cutting down of an isthmus is accomplished sometimes by

springs, but generally by atmospheric agencies; by wind, by rain, and by frost, the mightiest of all. But all these lose all their power when once the surface is brought down below water-level, for then, though springs may still issue, they no longer flow down channels; rain and wind then spend their force on the water; frost must be fearful if it makes a foot in thickness of ice. Then, indeed, a new set of erosive actions may come into play—waves, tides, currents. In lakes, however, waves are feeble, tides and currents generally absent; so in such cases the subaqueous bar will last indefinitely. Even on the sea the like may happen in sheltered places, such as fiords, gulfs, or deep bays. In exposed situations where waves are violent, and tides or currents strong, the process of destruction will continue, as it is continuing in Sark. Yet sometimes even in exposed situations, destruction may be delayed or arrested. On shores where much material is being carried along the coast, that is, in places where a groyne would arrest sand or shingle, there such a bar may also arrest them, and instead of being destroyed, may itself accordingly actually grow. A well-known example is the Chesil Bank. (An interesting and valuable paper by Mr. Wheeler, reviewed in GEOL. MAG., January, 1897, p. 44, should be consulted in reference to this motion of shingle.) Accordingly there is a natural tendency in many situations for bars or causeways to form, at or near the water-level, connecting islets or rocks with the mainland.

These considerations have some bearing on the force of two arguments adduced in two controversies of old standing. When discussing the antiquity of the present sea-level along the English Coast, it has been argued that the causeway to St. Michael's Mount and the low-tide access to Holy Island have remained unchanged since our earliest records. But the considerations of this paper show that upheaval or depression might be going on and yet these connections suffer no visible alteration through many centuries. Again, in the discussions on the Parallel Roads of Glen Roy, etc., their coincidence with various cols forms a strong argument in favour of the Ice-Dam-Lake theory, and against the theories of submergences. According to that theory, a glen, dammed at its mouth by ice, would fill with water to the level of a col, but at that level the rising waters would find an exit over the col; the col level would therefore fix the shore-level, so that a road should agree with a col. But on the opposing theory of submergences, why should the resulting shore-lines chance so regularly to coincide with the cols? This argument is very strong. Yet the considerations of this paper show that there would be some tendency for a col to be cut down to the sea-level and no further; to become as one of these bars; and so to coincide with a shore-line. These considerations, therefore, to some extent weaken that powerful argument. It is remarkable that Nicol and J. F. Campbell, who both discuss the coincidence of the beaches with the cols, seem neither to have noticed this point.

V.—The Dry Lakes of Western Australia.

By Harry Page Woodward, J.P., F.G.S., Assoc. M. Inst. C.E., F.R.G.S ;
Honorary Consulting Geologist and Mining Engineer to the Colony of Western Australia.

UPON glancing at the map of Western Australia, the eye is at once attracted by a series of large lakes in the interior; which are further often emphasized by being coloured blue. The natural inference is, that they represent sheets of water, either fresh or salt.

Now if these lakes contain fresh water, the surrounding country must be almost a paradise; and if salt, still the evaporation from them would cause the atmosphere to be humid, and hence there would probably be heavy dews or light showers of rain all the year round. But this is not the case, the map being entirely misleading, and no Government should continue to reproduce these incorrect charts: for in the first place, they are not lakes in the accepted sense; and in the second, they are not correctly mapped in; for in one instance alone, that of Austin Lake, the Island (where many a promising mine is being worked at the present time) is represented upon the maps as a sheet of water five miles wide. The reason for this discrepancy is that these lakes were invented by the early explorers, and have never since been altered upon the maps. This island is about one and a half miles long, and rises abruptly from the lake-bed to a considerable elevation—in fact, being one of the highest ridges of the district. It is divided both from the north and south shores by narrow boggy channels, each of which is about 400 yards in width, whilst from its high point no extensive lake is visible, but only a number of very shallow depressions fringed by sandy ridges. These are not lakes in any sense of the word, but a series of salt clay-pans which drain one into another, and thus in very wet seasons they serve to carry off the surface water; but since the fall of the ground is so slight, and the surface presented for evaporation so great, this water never finds its way to the sea.

To understand this so-called Lake District thoroughly, one must realize that the interior originally consisted of a large tableland about 2,000 feet above the sea-level, above which here and there rose low range-masses or ridges of hard hornblendic rocks, or bosses of granite, which are the fundamental rocks of the district. These rocks, now that denudation has exposed them, are seen to run in belts (often of considerable width) in a north-westerly and south-easterly direction; but whether they outcrop or not, their strike can be traced by the nature of the superficial deposits, which are the result to a great extent of their disintegration.

The present configuration of the surface may be described as due to belts of high sand-plains, while here and there in hollows, or around the margins of the more elevated areas, one meets with outcropping bosses of gneissic granite, which rock is found everywhere to underlie these high sand-plains. More or less parallel with

these, are areas of depression, in which the surface is for the most part covered with sand, loam, or clay; whilst from these valleys rise steep rocky ridges consisting of hornblendic schists, diorite dykes, and quartz reefs, which compose the auriferous series.

These valleys form the main drainage of the country, which falls in a south-easterly direction, but so gradually, that the rain-water, instead of cutting out a channel, spreads out in a broad sheet over the flat surface, thus forming, during a wet season, a series of large but extremely shallow lakes.

The water shed from the adjoining hills naturally, in rushing down their sides, carries with it not only such soluble salts with which it comes in contact, but also a certain amount of clayey matter, which is spread out in a thin layer over the plain, whilst the salts held in solution are deposited as the water evaporates, the result being the formation of numerous large salt clay-pans which have incorrectly been called lakes. These clay-pans, for the most part, run in chains, and are connected by channels—the whole being fringed by blown sandbanks containing a large quantity of gypsum, and upon which nothing but salt herbage, such as salt-bush, samphire, and mesembryanthemums, will grow. The beds of these pans are for the most part composed of a thin layer of clay, which rests directly upon the upturned edges of the highly inclined decomposed schists beneath. In other cases, the covering of clay is entirely wanting, the floor being apparently formed directly by the planed-down edges of the indurated rock, which is often strewn with fragments of quartz, whilst here and there huge reefs of this mineral outcrop at the surface.

When these lakes occur in hilly country, they generally cover the entire valley; but when on the open plain, they are fringed by æolian deposits, which often resemble coastal sand-dunes; whilst in other cases, where hills occur upon one side, the rocks of which are of a highly decomposed nature, these latter are found to be eroded in such a way that they form nearly vertical cliffs. Again, in the northern portion of the field, where these lakes are met with in tracts of country, the surface of which is covered by a deposit of indurated ferruginous clay (cement), they are surrounded by nearly vertical cliffs of kaolinized schists, capped by the above-mentioned recent deposits. In this class of country also, branches of these flats run off from the main valley, which gradually split up into canyon-like watercourses called "wash-aways." Immediately after heavy rain is the only time that the surfaces of the lakes may be seen covered by a few inches of salt water; and, in rare cases, even by a few feet when some obstruction to its flow has occurred. It must, however, be borne in mind that these remarks do not refer to the small fresh-water swamps and clay-pans, always spoken of as lakes, but to those large areas which are marked upon the plans as such.

As before stated, there is not sufficient slope in the surface of the country or a large enough rainfall to form rivers; also the surface-area exposed to evaporation is too great to allow any of the water to flow away; therefore, not only is there a thin layer of

sediment deposited upon each occasion that these flats are covered by flood-water, but also a quantity of salt.

It would be only natural to suppose that in countless ages, these deposits would assume a considerable thickness; but this is not the case, for do we not in many places find the upturned edges of the very oldest known rocks forming the beds of these lakes, and where they are covered, it is only by a thin deposit of clay. After looking into the reasons for this, we first notice that the salts deposited upon these surfaces consist very largely of gypsum, which upon crystallizing out in the clay causes it to split up, thus allowing the weather to act upon it, which quickly converts it into an almost impalpable powder, which the wind distributes far and wide over the surface of the country, whilst the sand and heavier particles are blown along the surface until they meet with some obstruction, where they accumulate to form sand-dunes, hills, and ridges.

It will naturally be asked why, if these lake-beds are formed by wind and not by water, they should assume such a dead level. The answer to this is—that the subterranean water is always so near the surface that the moisture rising from it by capillary attraction holds together the particles, thus preventing either the superficial deposits or the disintegrated rock below a certain level from being removed by the wind.

The formation of the cliffs around these lakes would probably start in the same manner that the "break-aways" are being formed at the present day, viz.: by the action of the water thrown from the hard impervious cement-hills, which has not only cut out the canyon-like channels through them, but also into the decomposed rocks below to the lake-bed level. The resulting débris from this source, as well as that which falls periodically from the cliff-faces, must then have been removed by the wind, because no trace of it remains upon the clean-swept rock-bottomed lake-bed. This action goes on year after year. The rain falling upon the surface of the ground dissolves out the salts which the water carries in solution, together with the insoluble sediment in suspension, redepositing them upon these plane surfaces where they dry, and efflorescence being possibly added to by a small portion of the disintegrated rock-surface of the bed itself, this fine material is again scattered over the surface of the land by the wind.

Thus the country remains salt, since no streams carry away the saline matter to the sea, whilst the underground percolation of the water is so extremely slow that in millions of years no appreciable difference in its saltness seems to have taken place.

The subterranean water beneath these lake-beds is abundant and extremely salt, often containing as much as forty ounces to the gallon, but it varies considerably in quality, for the further a well is removed from these salt clay-pans, and the higher the elevation attained, the fresher the water becomes; thus, beneath the high sand-plain, or the elevated pervious caps of disintegrated granite, perfectly fresh water is often met with because from them the salts have been completely leached out, whilst they are not resupplied, since their

distance from the lakes is too great for the wind to carry the salt. That salt is conveyed in this manner, is clearly proved in many places where granite rocks outcrop in close proximity to salt lakes. Upon these rocks, holes and depressions are met with, in which any rain-water that may fall is caught; and it was from this source that the original explorers and prospectors obtained a supply of water. It was always considered a certainty that such water would be absolutely fresh, being in fact pure rain-water, but there are exceptions to this rule, for at one place upon a rock, which rises at least 400 feet above the lake-bed level, a flat rock hole was found to be quite salt, in spite of the fact that it held only a few hundred gallons, and was thoroughly washed out and replenished with each fall of rain. It is clear, therefore, that the salt was first blown upon the rocky surface, and then dissolved and carried by the water into the rock hole, since there is no other source from which it could have been supplied.

It will be seen from the above that the so-called lakes have no claim to that title, but are merely salt wind-planed flats; and that they do not exist in reality as depicted upon the maps; therefore it is high time their name was changed, since it is most misleading; and further, all map publishers should cease to represent them by a symbol which conveys the idea that these dry æolian flats of Western Australia are lakes filled with water.

REVIEWS.

I.—Papers and Notes on the Genesis and Matrix of the Diamond. By the late Henry Carvill Lewis, M.A., F.G.S., etc. Edited from his unpublished MSS. by Professor T. G. Bonney, D.Sc., F.R.S., etc. Large 8vo; pp., including index, 72. Two plates and illustrations, chiefly of minerals, in the text. (London: Longmans, 1897.)

THE late Professor Carvill Lewis is best known to English geologists from his work on the glacier formations of our country—work which has been eminently suggestive of new interpretations, and which a particular school of glacialists has not been slow to utilize. On the other hand, as a Professor of Mineralogy in the Academy of Natural Sciences at Philadelphia, we must regard him as having been well qualified to investigate the interesting phenomena presented by the diamond-mines round Kimberley. This work was in a great measure based upon specimens shown and information obtained at the Colonial Exhibition in South Kensington as long ago as 1886, when the De Beers mine had perhaps a depth of 500 feet. It is true that mining operations have now extended to a depth of 1,500 feet, and thus it might be thought that Professor Lewis' papers are a little out of date. Excepting, however, that the main mass of the diamond-rock is harder and less easily disintegrated, there does not appear to be such a difference between 500 feet and 1,500 feet as one might

suppose, whilst in many respects the old quarrying system afforded more facilities for observation than underground mining.

Two papers were read before the British Association in 1886 and 1887 respectively, and abstracts of these were published. Hence, Sections I and II of the present work, representing those papers, are not wholly original; and, indeed, Lewis' conclusions have been quoted, notably by Mr. Sawyer, the Government Inspector of Mines, and possibly by other writers on the diamond-mines of South Africa. The history of a rock somewhat similar to that prevailing in the Kimberley pipes, which has been found at two localities in the United States, constitutes Section III of this posthumous publication.

The MSS. of these three papers ultimately came into the hands of Professor Bonney, who, at the request of Mrs. Carvill Lewis, has kindly undertaken to edit the whole. The two first papers are printed very nearly as they were left by Professor Lewis. The statements made and the results given represent his views in 1887, and, so far as the editor knows, at the time of his death (July, 1888). Of the third paper (Section III) nothing had been written, the materials consisted only of some very brief "jottings" and a small set of rock-specimens. The editor has drawn up a statement of the facts concerning them from such literature as he has found, adding brief descriptions of the structures of the specimens themselves. Some other material remained, from which data two additional sections were compiled, but it was finally decided to cancel them. In these (note, p. 68) the author insists on the intimate connection of peridotite (or serpentine) with the diamond at least in South Africa, and also in Kentucky.

Our readers will easily understand the fascination which such a subject as the diamond-rock of South Africa would have for a man of Professor Lewis' stamp. Truly, it is a rock *sui generis*, and the scientific interest attaching to it is only exceeded by its commercial value. The author, in quoting previous writers, does no more than justice to Mr. Dunn, who, as long ago as 1873, when many foolish speculations were in vogue, insisted on the "euphotide (?)," as he called it, being the *mother rock* of the diamond.

It may be that there was a little confusion at the time of Lewis' writing (1886–7), between the rock described in Section I and that described in Section II, the one without diamonds, the other diamantiferous. Nothing can be more certain than that the real diamond-carrying rock of the Kimberley district is a medley or mixture, and in that case is scarcely entitled to a specific name, such as "Kimberlite," bestowed by the author. "Kimberlite proper," he tells us subsequently (p. 51), "is a typical porphyritic lava"; yes, but without diamonds. This, we apprehend, is the rock described in Section I as "Hard Wack Floating Reef," and is the same rock as that familiarly known as the "snake" of De Beers mine, a winding dyke of igneous rock varying in width, which Mr. Williams, the General Manager of the De Beers Company, supposes to be a younger eruptive formation coming from the same

volcanic source as the "blue ground." Olivine is the main ingredient of this rock, the principal accessories being enstatite and biotite. It would have been satisfactory if a detailed examination of this rock had been given, since it most probably represents, to a considerable extent, the original peridotite, which bursting through the carbonaceous shales of the Karoo beds, at intervals, has supplied a large portion of the mixture, with a serpentinous base, known as the diamond-rock, or as Lewis would call it "Kimberlite breccia."

The actual diamond-rock itself, being so much of a mixture, the mineralogical details given in Section II are of less importance, since it is difficult to determine which minerals are endogenous and which are derivative. Most of the minerals described, including the diamonds, Lewis seems to have regarded as endogenous. We can easily believe that chrome-diopside, smaragdite, bastite, magnetite, chromite, ilmenite, and picotite, said to be common under the microscope, represent paragenetic minerals of the orginal peridotite, as also perofskite, said to be the most abundant titanium mineral of ultra-basic rocks. The same may also be said of the peculiar brown biotite (altered to vaalite) and abundant specimens of pyrope. When we bear in mind, however, that Moissan is said to have detected more than eighty species of minerals in the "blue ground," or diamond breccia, we can well believe that many of these are derivative.

The minerals described in Section II are said to lie, some of them porphyritically, in a base which is a more or less homogeneous serpentinous mass containing fragmental enclosures, of which shale is the most abundant. The fragments of shale have lost their shaly character together with their carbonaceous matter, a result, he considers, of alteration through the heat of the lava. It should be noticed, in this connection, that local writers have usually spoken of the shale in the diamond-rock as not being fused at the edges. As regards the composition of the ground-mass, after eliminating from the calculation the very considerable quantity of calcite, he concludes that one-half consists of serpentine and the bulk of the remainder of olivine.

Let us now see how far the editor assists us in comprehending the diamond-rock of the Kimberley district. Plate i is an effective picture of a fragment of the diamond-rock of De Beers mine, from rather over 1,000 feet (obtained by Stone in 1894). This presents a pseudo-porphyritic appearance: the largest inclusion, nearly a couple of inches long, consists of rather decomposed compact serpentinized olivines and pyroxenes, with a few flakes of mica. In plate ii (1) a slice of this rock is magnified twenty-two diameters; crystals of mica and altered enstatite are seen with several grains of serpentinized olivine, whilst the separate mineral grains decrease in size until they become indistinguishable from the granular matrix of serpentine, calcite or dolomite, iron-oxide, perofskite, etc. We may well believe with the author that, in his time, there was no named rock type having at once the composition and structure of the Kimberley rock. But, bearing in mind that the diamond-rock

is essentially a mixture, the name "Kimberlite," as applied to this rock, though it may be convenient as a mere designation, can scarcely possess any scientific value.

The so-called porphyritic brecciated structure Lewis attributed not to pressure, but to mechanical disturbances during and after cooling, and according as the porphyritic or brecciated structure predominated so did people differ in their opinion as to whether it was a lava or a tuff. He assigns nine reasons why it should be a true igneous lava, and not a mud or ash. In this respect he had in mind the mud volcano theory which was held by Erskine. Certainly the diamond-rock itself is not exactly a true igneous lava, whatever portions of it may have been; but the "floating reef" and "snake" of De Beers, which Lewis probably would call "Kimberlite proper," better retain the character of igneous erupted masses. These unfortunately are not diamantiferous.

There can be little doubt, as long ago pointed out by Chaper, that these pipes bear the marks of several eruptions, as of course must have been the case in volcanic necks. There is nothing in this which should cause the Kimberley pipes to differ from other necks. It is the presence of diamonds in association with broken-up peridotites which constitutes at once the interest and the peculiarity.

Specimens from two localities in the United States, described in Section III, are said to bear a close resemblance to the diamond-rock of South Africa. These are peculiar peridotites, which, in spite of so much of the structure being secondary, preserve the shape of crystals, olivine and enstatite being the most conspicuous together with the peculiar brown biotite already mentioned. These rocks occur in dykes which have been forced through beds of Carboniferous age: they contain fragments of shale, and now consist mainly of serpentine with a considerable proportion of carbonates. Plate ii (2) is a magnified section of a portion of one of the American rocks, the matrix consisting of granules of serpentine, iron-oxide, perofskite, etc., along with crystals of biotite, as in the Kimberley rock, whilst the grains are mostly olivine in various stages of change. The editor remarks that the specimens, both from New York State and from Kentucky, are perhaps a little more like a porphyritic igneous rock than anything he has seen from Kimberley, owing to the absence of distinct rock fragments, and the more uniform size of the included minerals. No diamonds are found, but carbonadoes are said to occur in the Kentucky rock.

With respect to the Kimberley rock, the editor, whilst admitting that it differs from any rock, whether peridotite or serpentine, does not agree with the author in his conclusions as to its origin. Professor Bonney considers that the apparent glassy inclusions, "together with all the large minerals (not excluding the diamonds), are true 'fragments,' like the pieces of shale which are sometimes found associated with them, produced by the explosive destruction of more coarsely crystalline rocks of earlier consolidation, and not the result of a fluxional movement in a magma, which had previously reached a stage of partial separation and incomplete consolidation,

and had in addition incorporated fragments of overlying rocks in its upward progress." But he allows that this interpretation would not necessarily affect his friend's views as to the genesis of the diamond, viz., by the action of an extremely basic rock upon carbonaceous material.

Although there seems to be a pretty general consensus of opinion that, in some way or other, a peculiar peridotite, invading carbonaceous beds, is the mother rock of the diamond, yet the evidence at present is by no means conclusive either as to the depth or the precise conditions under which this action took place. That the diamonds originated lower down than the position in which they are now found, is most probable, since the breccia in which they occur is more or less of a mixture which has been forced upwards. The evidence as to depth is somewhat contradictory; for, on the one hand, we have the fact that the several pipes have their own type of diamonds, a circumstance tending to indicate that the diamonds originated in each pipe separately, and not in a general reservoir. On the other hand, we have the fact that the area within which diamond pipes may be expected to occur is of wide extent, since the Leicester mine is forty miles from Kimberley and Jägersfontein sixty miles. This might be held as pointing to a general reservoir of diamantiferous rock seated at a great depth. On the whole it would seem that, although the peridotite may be widely distributed, at a great depth, the conditions under which it becomes diamantiferous are local and only occur in the pipes.

The chemical aspect of the question, though not dealt with in the present publication, has greatly changed since Professor Lewis wrote. For many years chemists looked upon hydrocarbons as the most promising material from which the diamond might have been derived, and laboratory experiments were conducted on this supposition. As bearing on this point, it is interesting to note that Roscoe by digesting the "blue ground" with ether obtained a hydrocarbon which was found to be crystalline and strongly aromatic, burning with a blue flame and melting at 50° C. (p. 51).

More recently, as is well known, Moissan and other chemists have actually obtained small diamonds by melting iron mixed with carbon in the electric furnace, and arranging the experiment so that the separation of the iron from the carbon takes place under enormous pressure. This reaction consequently is now regarded as the one most likely to have produced diamonds in nature. "It is certain," said Sir William Crookes, at a recent lecture before the Royal Institution, "from observations made at Kimberley, corroborated by experience gained in the laboratory, that iron at a high temperature and under great pressure will act as the long-sought solvent for carbon, and will allow it to crystallize out in the form of diamond—conditions existent at great depths below the surface of the earth."

Undoubtedly the fact that small diamonds have been made by the action of iron on carbon favours the supposition that this, too, may be Nature's process, and there is further corroboration in the fact that iron-oxide is the most abundant substance of the ash of

Junction of amygdaloid basalt with shales and limestones, Sheare, half a mile east from Kingborn, Fife. Geikie's "Volcanoes," vol. i. p. 464.
From a photograph by Mr. R. Lunn.

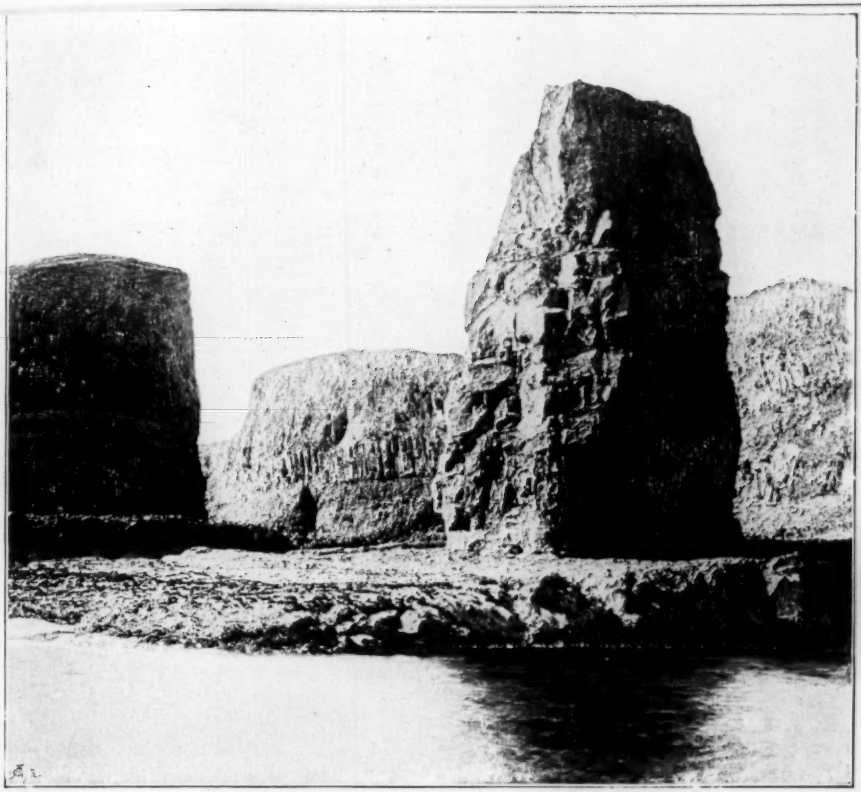


FIG. 1.—View of Dûn Beag, Sanday, seen from the south. Geikie, vol. ii, p 230.

Junction of amygdaloid basalt with shales and limestones, Shore, half a mile east from Kingborn, Fife. Geikie's "Volcanoes," vol. i, p. 464.
From a photograph by Mr. R. Turnbull.

diamond. Moreover, microscopic diamonds have been found in meteoric irons. Yet it must be regarded as a singular circumstance that basalts far richer in iron than the Kimberley diamond-rock have passed through beds containing carbonaceous matter, and contain large masses of metallic iron which is not associated with diamond either at Kimberley or elsewhere.

It must be a matter of regret to geologists, and indeed to all men of science, that Professor Lewis was not spared to continue his investigations on a subject which presents so many points of interest.

II.—The Ancient Volcanoes of Great Britain. By Sir Archibald Geikie, F.R.S., D.C.L., D.Sc., etc., Director-General of the Geological Survey. Imperial 8vo. In two volumes. Vol. I, pp. xxiv and 477, with four maps and 175 illustrations. Vol. II, pp. xvi and 492, with three maps and 382 illustrations. (London: Macmillan & Co., Ltd., 1897. Price 36*s*. nett.)

(Second notice.)

(WITH PLATES XVI AND XVII.)

ALTHOUGH we have no "burning mountains" in England, and no evidence of volcanic action but such as belong to the remote past, yet a careful study of modern volcanoes and of the products of eruptions observed all over the world, has given us the key by which to interpret the ancient tuffs, lavas, basalts, gabbros, andesites (porphyrites), trachytes, pitchstones, felsites, rhyolites, etc., which are met with in these Islands, and also to detect the old plateaux discharges of volcanic materials of Carboniferous age that now form broad tablelands or ranges of hills; or the puys, whose ejected materials formed cinder-cones or lava-domes and were confined to smaller areas round the centres of eruption, with occasional flows of basic lava, as admirably displayed in Central France. To these must be added the innumerable examples of dykes, volcanic necks, bosses, and sills, exposed by denudation, all of which have been carefully studied and classified.

With the ordinary type of modern, and especially of active, volcanoes, we need not here concern ourselves. The lofty cones of the Vesuvian type, with their widespread lavas and ashes, their vast craters, and numerous parasitic vents, can form no lasting record of volcanic action. Subaerial denudation, acting incessantly year after year, gradually planes down and carries away their unstable slopes, and if unchecked by any fresh discharge of volcanic material, degradation will at last have removed the whole cone. But whilst no universal rule can be laid down as to the relative durability of any rock, yet in the course of vast periods of time during which denudation and disintegration have been carrying on their destructive work, the older eruptive masses, consisting usually of the more durable materials, have at last risen into prominence, and now form some of our wildest scenery and the summits of our most craggy and spiry mountains. Among the many examples given, we cannot forbear to quote one illustration from the chapter entitled the "Influence of Volcanic Rocks on the Scenery of the Land," which is in Sir A. Geikie's most agreeable style—

"A contrast of another type meets us in the broad midland valley of Scotland. Around the city of Edinburgh, for instance, the landscape is diversified by many hills and crags, which show where harder rocks project from amidst the sediments of the Carboniferous system. On some of the crags the forts of the early races, the towers of Celts and Saxon, and the feudal castles of the Middle Ages, were successively planted, and round their base clustered for protection the cots of the peasants and the earliest homesteads of the future city. Beneath these crags many of the most notable events in the stormy annals of the country were transacted. Under their shadow, and not without inspiration from their local form and colour, literature, art, and science have arisen and flourished. Nowhere in short, within the compass of the British Isles, has the political and intellectual progress of the people been more plainly affected by the environment than in the central district of Scotland."

"When now we inquire into the origin and history of the topography which has so influenced the population around it, we find that its prominences are relics of ancient volcanoes. The feudal towers are based on sills and dykes and necks. The fields and gardens, monuments and roadways, overlie sheets of lava or beds of volcanic ashes. Not only is every conspicuous eminence immediately around of volcanic origin, but even the ranges of blue hills that close in the distant view to south and north and east and west are mainly built up of lavas and tuffs. The eruptions of which these heights are memorials belong to a vast range of geological ages, the latest of them having passed away long before the advent of man.

"But they have left their traces deeply engraved in the rocky framework of the landscape. While human history, stormy or peaceful, has been slowly evolving itself during the progress of the centuries in these fertile lowlands, the crags and heights have remained as memorials of an earlier history when Central Scotland continued for many ages to be the theatre of vigorous volcanic activity." (Vol. i, pp. 104, 105.)

The Puys.—As an illustrative example of the Carboniferous puys of Scotland, we give that of the junction of the amygdaloidal basalt with the shales and limestone half a mile east from Kinghorn, Fife. (Plate XVI.)

"When the puys began their activity, this district was gradually dotted over with little volcanic cones. At the same time it was affected by the general movement of slow subsidence which embraced all Central Scotland. Cone after cone, more or less effaced by the waters which closed over it, was carried down and buried under the growing accumulation of sediment. New vents, however, continued to be opened elsewhere, throwing out for a time their showers of dust and stones, and then lapsing into quiescence as they sank into the lagoon. Two groups of volcanoes emitted streams of lava and built up long volcanic ridges—those of Fife and West Lothian.

"The occasional presence of the sea over the area is well shown by the occurrence of thin bands of limestone or shale, containing such fossils as species of *Orthoceras, Bellerophon,* and *Discina,* which

suffice to prove the strata to be the equivalents of the Lower Limestone shale and part of the Carboniferous Limestone of England" (pp. 463–4).

One of the most interesting features of the massive relics, left by denudation of ancient volcanic lavas, is to be seen in the Basalt-plateau of the "Parish of Small Isles," which includes the islands of Eigg, Rum, Canna, Sanday, and Muck.

"That the fragments of the basaltic plateau preserved in each member of the group of the Small Isles were once connected as a continuous volcanic plain, can hardly be doubted. Indeed, as already stated, they were not improbably united with the plateau of Skye on the north, and with that of Mull, Morven, and Ardnamurchan on the south. Taking the whole space of land and sea within which the basalt of Small Isles is now confined, we may compute it at not much less than 200 square miles.

"The most varied and interesting of the fragments of the basaltic plateau in the area of the Small Isles is that which forms the island of Canna, with its appendage Sanday. Canna measures five miles in length by from half to a mile in breadth, and consists entirely of the rocks of the plateau and their accompaniments. The basalts are exposed along the north coast in a range of mural precipices rising to a height of about 600 feet above the sea. From the top of the escarpment the ground falls by successive rocky terraces and grassy slopes to the southern shore-line. Sanday, connected with the large island by a shoal and foot-bridge, is two miles long and 220 to about 1,200 yards broad. Its highest cliffs range along its southern shore to a height of 193 feet, whence they slope gently northward into the hollow between the two islands."

The author had observed, as far back as 1865, an ancient river-channel, which, during the volcanic period, had been eroded on the surface of the basalt-plateau, and of which a small portion had been preserved under a stream of pitch stone-lava that had flowed into and buried it.

This watercourse was shown to have been excavated by a stream coming from the north-east, or east, and to be younger than the plateau-basalts of the district or even the dykes which cut the basalts. Yet that it belonged to the volcanic period was proved by the manner in which it had been sealed up and preserved under the black glassy lava of the Scuir. Other and more abundant evidence of river-action was brought to light by Sir A. Geikie, during his later examination of the islands of Canna and Sanday, belonging to an earlier part of the volcanic period. This evidence reveals that a powerful river, flowing westwards from the Highland mountains, swept over the volcanic plain while the sheets of basalt were still being poured forth, and while volcanic eruptions were taking place from cones of slag. In one spot, the former existence of an old land-surface is attested by the presence of some shaly layers, which have been deposited on a remarkable slaggy basalt; above these is a thin coaly parting enclosing a small tree stump still standing erect upon the coaly parting and shale, which in its turn is immediately overlain by and enveloped in a massive flow of basalt.

Again, "In the Creag nam Favileann (Seamews' Crag) and the gully that cuts its eastern end, likewise in the two singularly picturesque stacks of Dùn Mòr and Dùn Beag, or Big and Little Gull Rocks [Pl. XVII, Figs. 1, 2], which here rise from the foreshore, two distinct platforms of detrital material may be noticed among the basalts. Both of these can be well seen in Dùn Mòr, about 100 feet high [see Fig. 2].

"The lower band, four or five feet thick, is here a rather coarse conglomerate, which lies upon a sheet of scoriaceous basalt that extends up to the base of the Creag nam Favileann. It is directly overlain by another basalt, about 30 feet thick, which dips seawards and forms a broad shelving platform whereon the tides rise and fall. On this stack a second coarse conglomerate, about 10 feet thick, forms a conspicuous band about a third of the height from the bottom; it is composed mainly of well-rounded blocks of various lavas up to 18 inches or more in diameter, but it contains also pieces of Torridon Sandstone. It is covered by about 60 feet of basalt, which towards the base is somewhat regularly columnar, but passes upward into the wavy, starch-like, prismatic structure."

The story of Dùn Beag is still more interesting and remarkable, but in order to explain its structure properly one ought to have had at least three figures, figs. 272, 273, and 274; but those who are really interested will get the book and read it for themselves.

It is these and such like megalithic monuments which enable us to grasp the fact to what an enormous amount of denudation the old volcanic outflows must have been subjected. Referring to the effects of denudation Sir A. Geikie writes:—"The original height of the plateau of Mull is shown by the outlier of Ben More to have been at least 3,200 feet. If to this figure we add the portion of the basalt-group submerged under the sea, the height will probably be increased by several hundred feet. . . . . If, in the meantime, we suppose the present mean level of the plateau to be 1,000 feet above the sea, the difference between this amount and the assumed original height will be 2,000 feet. If, further, we take the present average rate of degradation of the Mull plateau to be $\frac{1}{6000}$ of a foot in a year, which has been shown to be probably a fair estimate, then the time required for the lowering of the Mull plateau from its original to its present average level amounts to twelve millions of years. Yet this period, vast though it be, does not carry us back even as far as the beginning of Tertiary time" (pp. 461–2).

"Among the broad features which soonest arrest attention in a survey of volcanic action is the tendency of volcanoes to range themselves along continental borders or in oceanic islands. This energy in Britain has shown itself along the western or Atlantic margin of the European continent.

"Moreover the volcanic rocks in Britain are ranged along the greatest length of the group of islands, in a general north-and-south line, from the south of Devonshire to the far Shetlands, and it is always on the western side of the country that they occur.

"A second and still more remarkable feature in the geological

history of Western Europe is the persistence of volcanic activity along the site of the British Isles . . . . that from primæval time, vaguely termed Archæan, onward to that of the older Tertiary clays and sands of the south-east of England—that is to say, through by far the largest part of geological history, as chronicled in the stratified crust of the globe—this long strip of territory continued to be intermittently a theatre of volcanic action. Every great division of Palæozoic time was marked by volcanic eruptions, sometimes over tracts hundreds of square miles in area and on a colossal scale. After a long period of quiescence during the Mesozoic ages, the renewed outbreak of volcanic energy in older Tertiary time, so marked over the western half of Europe, reached its maximum of development along the Atlantic border from the north of England and Ireland, through the chain of the Inner Hebrides to the Faroe Islands, Iceland, and Greenland" (p. 467).

These are at best but samples, drawn at a venture, from the vast stores of valuable information which have been garnered in these volumes by their author. Under his guidance we may study the various forms and behaviour of eruptive rocks, whether uprising as lava-flows on the surface, or, when the vents of volcanoes became plugged up, the final efforts which have led to the intrusion of sills and dykes, not only into the rocks beneath the volcanic sheets, but also in many instances into at least the older parts of the sheets themselves. And we may learn how these subterranean manifestations of volcanic action may be seen and recognized in almost every district.

Nor has the nature of the volcanic products of eruptions been left unstudied or unrecorded, and the student may gather much as to the behaviour of eruptive rocks in the field on a large scale, and if their microscopic structure is not so extensively dealt with, yet sufficient is said upon microlites, perlitic and spherulitic structures, granophyres, dolerites, etc., to satisfy all but the most exacting and enthusiastic of petrologists.

In closing this notice of the Ancient Volcanoes of Britain we cannot fail to congratulate the author on the very splendid series of illustrations with which the pages of this admirable work abound. The maps and printing are excellent, and evidently the publishers have determined to make the book a complete success. Every geologist who can spare 36*s.* will secure a copy for his private library; every public library and book-club will order a copy; of course Mudie's will take several! and we venture to believe they will be read by many.

REPORTS AND PROCEEDINGS.

Geological Society of London.

I.—June 23, 1897.—Dr. Henry Hicks, F.R.S., President, in the Chair. The following communications were read:—

1. "Notes on a Collection of Rocks and Fossils from Franz Josef Land, made by the Jackson-Harmsworth Expedition during 1894–6." By E. T. Newton, F.R.S., F.G.S., and J. J. H. Teall, Esq., M.A., F.R.S., V.P.G.S.

A large collection of rocks and fossils, obtained by the members of the Jackson-Harmsworth Expedition, chiefly from the neighbourhood of Cape Flora, on the south-west of Northbrook Island, but also from more distant localities visited during boat- and sledge-journeys, have been sent to the Director-General of the Geological Survey, and examined by the authors.

After a summary of what was previously known of the geology of Franz Josef Land, an account of the new specimens is given. The rocks are for the most part basalts and are described in detail; they are usually formed of labradorite, augite, and interstitial matter which is sometimes represented by palagonite containing a large percentage of iron-oxide. This palagonite is regarded as the hydrated representative of the residual magma left after the separation of labradorite and augite; and the conclusion is reached that in this case progressive crystallization has resulted in the concentration of iron-oxide in the mother liquor.

Most of the fossils have been collected around Cape Flora. The presence of *Ammonites macrocephalus*, *A. modiolaris*, and *Belemnites Panderi* indicate the presence of rocks of Lower Oxfordian or Callovian age; while, apparently above these, a plant-bed was met with in which the genus *Ginkgo* (*Salisburia*) is conspicuous, and this is believed to be of Upper Jurassic age.

The oldest fossiliferous bed yet found occurs about twenty miles to the west of Cape Flora, and also contains plant-remains, which, it is thought, may be Lower Jurassic and possibly of the age of the Great Oolite. The plant-beds and numerous indications of layers of lignite seem to show that these Jurassic strata are to a great extent of estuarine or fresh-water origin.

The general structure of the country appears to be typified by what occurs at Cape Flora, where cliffs of sedimentary strata some 600 feet high (for the most part hidden by talus) are overlain by 500 feet of basalt. At some other localities, however, the basalt is found at the sea-level.

It is pointed out that the islands which make up the archipelago of Franz Josef Land, are fragments of a formerly extensive region of plateau-basalts, similar to that of which the Færöe and the Western Isles of Scotland must have formed a part.

2. "Deposits of the Bajocian Age in the North Cotteswolds.—I. The Cleeve Hill Plateau." By S. S. Buckman, Esq., F.G.S.

This paper deals with a portion of the Northern Cotteswolds termed the Cleeve Hill plateau; and, like a previous communication on the Mid Cotteswolds, it is concerned with the deposits which intervene between the Upper Freestone and the Upper *Trigonia*-grit. The author shows that in the Cleeve Hill plateau there is, beneath the Upper *Trigonia*-grit, a further series of beds which have not been found elsewhere in the Cotteswolds, and by placing them in their order he is able to show the sequence of the Cleeve Hill "Inferior Oolite" rocks, about which there has been so much misconception. A map is given to show the positions of the different quarries, with notes as to the strata which they exhibit.

The paper also gives further information concerning the Bajocian denudation, and with it is presented a map showing the areas of the different rocks upon which the Upper *Trigonia*-grit reposes non-sequentially. Notice is also taken of a water-bearing bed, and of its economic importance. Some remarks are made upon the ancient geography of the Cleeve Hill plateau in regard to streams. Their probable courses are marked upon a map.

3. "Pleistocene Plants from Casewick, Shacklewell, and Grays." By Clement Reid, Esq., F.L.S., F.G.S.

The plants from Casewick and Shacklewell were obtained by washing two lumps of clay in the collection of the late Sir Joseph Prestwich. The species are few and call for little remark, except that the climate was not Arctic. They are all common British forms.

The collection from Grays consists of leaves, already partly determined by Gaudin and Heer, though unpublished, and some lumps of clay, out of which the author washed a few seeds. The flora points clearly to a temperate climate and mild winters.

4. "An Explanation of the Claxheugh Section (Co. Durham)." By D. Woolacott, Esq., M.Sc. (Communicated by Professor G. A. Lebour, M.A., F.G.S.)

The section of which an explanation is offered in this communication occurs about two miles west of Sunderland, and has been noticed by Messrs. King and Howse and Professor Lebour. The base shows the Permian Yellow Sands, which are succeeded at the west end of the section by the Marl Slate, thin-bedded limestones, and at the top crystalline limestones without any trace of bedding. At the east end the Marl Slate and thin-bedded limestone are absent, and, except when a breccia intervenes, the crystalline limestone rests on the Yellow Sands, though the thin-bedded limestone and Marl Slate show no signs of thinning-out. There are also minor complications. The author suggests that the section may be explained by supposing that denudation occurred in a cavern, the roof of which afterwards fell in, and that disturbances were also produced by "creep"-movements.

CORRESPONDENCE.

THE ORIGIN OF THE GREY GNEISS OF ANGLESEY.

Sir,—In the debate at the Geological Society on April 28, when my paper on the Grey Gneiss of Anglesey was read, some criticisms were offered by the Rev. J. F. Blake, to which, with your permission, I desire to make a brief reply. Had I enjoyed the good fortune to be present on the occasion, it would not be necessary for me thus to trouble you.

To save your valuable space, I will confine myself to one point—the nature of the rock out of which the grey gneiss has been formed. This is a vital part of the question, and, if it be settled, much else will follow. In my paper I contended that the gneiss was originally

a felsite. The Rev. J. F. Blake denied this. He admitted that "some eminent petrologists" had, on microscopic examination, pronounced the rock from which the gneiss has been formed to be a felsite; but he would not yield to their testimony. He had seen the rock in the field, and he "regretted" that these authorities had made a mistake. It may seem a little presumptuous for Mr. Blake to contest a determination made by three of the best petrographers in Europe; and we may suspect that if Ajax ventures to defy the lightning, Ajax will not do himself much good. Most people will consider that such an authoritative identification is absolutely decisive, and I, for one, assume it to be so. My main purpose in writing is, however, to point out that even the field-evidence, which, in Mr. Blake's opinion, refutes the "eminent petrologists," is dead against him. Within a hundred yards of the critical section at Y Graig, in which the felsite passes into the gneiss, a rock which closely resembles the felsite and which also graduates into a similar gneiss, is seen to penetrate the diorite, which is more or less altered, in numerous veins, some of which are branched. Thus the microscope of the "eminent petrologists" and the eyes of the field-observer who knows where to look, lead to the same conclusion. Mr. Blake himself admits that the rock thus shown to be a felsite "passes by insensible gradations into the ordinary grey gneiss." He says that the "phenomenon is a purely local one." This is a mistake, as I can prove by examples to the contrary, though the felsitic structure is nowhere, I believe, so marked as at Y Graig; but if it were so, my case is not materially affected. If felsite is converted into a typical gneiss at Y Graig, all *primâ-facie* objection to such a change disappears, and the field-evidence receives important confirmation. C. Callaway.

Tewkesbury.
May 11, 1897.

TRINUCLEUS SETICORNIS AND THE UPPER BALA BEDS.

Sir,—In the report of a discussion of a paper on the fauna of the Keisley Limestone, Part II (Q.J.G.S., vol. liii, p. 106), the author is stated to have made the following remark: "Mr. Marr . . . . denied that *Trinucleus seticornis* was specially characteristic of Upper Bala beds, though some years ago he had called the Upper Bala beds of the Haverfordwest area after that fossil." I should hardly have supposed that my words required an explanation, but as the speaker on that occasion appeared puzzled, I will give it.

Mr. T. Roberts and I (in 1885) called the Upper Bala beds of Haverfordwest *Trinucleus seticornis* beds, because the fossil or one of its varieties is particularly abundant in those beds *in that area*, and we did not there discover it in beds of Middle Bala (Caradoc) age. We were perfectly justified in our action, and there is precedent for it; e.g., no one supposes that the *Leda myalis* beds of the Cromer Forest Group are the only beds with *Leda myalis*. If we had spoken of the beds as constituting the zone of *Trinucleus seticornis* an explanation might be necessary, although I hold that even that

would be admissible. I certainly supposed that all students of Lower Palæozoic geology knew that the fossil which has been determined as *Trinucleus seticornis* by Salter, Angelin, and many others occurs abundantly in Middle Bala (Caradoc) beds. If Mr. Reed will turn to my Sedgwick Essay "On the Classification of the Cambrian and Silurian Rocks" (published in 1883), where the Upper Bala beds are generally separated from those of Middle Bala age, he will find *Trinucleus seticornis* recorded as a Middle Bala fossil in North Wales (p. 39), the Lake District (p. 57), and Scandinavia (p. 76), and nowhere recorded in the Upper Bala list. Mr. Roberts and I used the term *T. seticornis* beds locally because we thought (wrongly it appears) that under the circumstances it could mislead no one.

In the above-mentioned discussion I disputed the statement that *T. seticornis* was a characteristic Upper Bala fossil, as the form which is usually taken as *T. seticornis* occurs, as remarked above, in Middle Bala beds. I have not seen Hisinger's original specimen, and have been unable to obtain access to a copy of the later (1840) edition of "Lethæa Suecica" in which the form is figured. If Mr. Reed has examined Hisinger's specimen, and can prove that the reference by Angelin, Salter, Linnarsson, Törnquist, Tullberg, and others of the common Middle Bala form to Hisinger's species is erroneous, I will cry "Peccavi." I am quite prepared to believe that there is a characteristic *Trinucleus* in the Upper Bala beds, but doubt whether it is Hisinger's species. I should not be surprised if the variety *Bucklandi*, amongst others, be eventually proved characteristic of these beds.

There are many statements in the body of Mr. Reed's paper on the Keisley Limestone with which I regret that I am unable to agree, but I do not wish to trouble your readers with questions of detail, especially as I am given to understand that the fauna of the Keisley Limestone is about to be examined by a very competent palæontologist.

JOHN E. MARR.

CAMBRIDGE, *July* 15, 1897.

ON THE ORIGIN OF LYNCHETS.

SIR, — Mr. E. A. Walford's article, which attempts to give a natural origin of lynchets, based on geological conditions, is not very convincing, and seems indeed to refute itself. Had he seen the many perfect examples which the Chalk Downs afford, I think he would never have suggested a natural explanation for lynchets. Joining the high road from Amesbury to Salisbury and close to the Workhouse are some very typical lynchets, all in the Upper Chalk, and yet confined to only a very small portion of it; yet, if geologic conditions are the cause, they should occur in many other localities of the Chalk area. The opponents of the artificial origin must prove why they are confined to small areas when the geologic conditions, which they consider as the cause, occur over wide areas.

Taking the Amesbury examples as typical ones, they will be seen

all to start from one straight line drawn about the middle of the slope, and to end, diminishing gradually in width, on the steep side of the slope. Their ground-plan also is *always* the same, and that is the shape of the blade of a scythe, and they end against one gradual curve, viz., the steep side of the slope, and are as nearly one size as the shape of the slope will allow.

When they occur on the slopes of escarpments and run along combes, these terraces will be seen to be parallel with the sides of the combe; and yet when they occur on the spur or slope separating two combes they will be found to run almost at right angles to those running up the combe, and right across the slopes, always commencing at one straight line and having the curved termination before alluded to. This, I think, shows conclusively that *natural* causes could not result in two sets of opposing terraces occurring under precisely similar geologic conditions. These artificial terraces, too, have in many cases quite altered the natural shape of the valley or combe, turning a $\wedge$-shaped valley into a $\cap$ one, as at Heddington, near Devizes. Again, the walls of these terraces may often be found, when cut into, to be faced with stones; flints in some areas, and sarsens in others, and these walls have, from this reason, been resorted to for the sake of the facing material.

This facing seems to me to have been a gradual process, and is another proof of the artificial origin of the terraces, which go as far back as the Common Field System, as Mr. Seebohm has so well shown in his book on the "English Village Community." The holdings in these fields were divided into strips all of the same length, forty rods long and four rods wide when they were acre strips, and two rods wide when they were half-acre strips. Now a boundary, of course, was necessary to separate strip from strip, and this boundary on the flat land consisted of a strip of turf, or a "balk," or a "mere," as it was termed.

Now in the case of the hill slopes it would not be so easy to keep a well-defined boundary, owing to the wash of the soil when cultivation began. No doubt to begin with, the strips were marked by cutting into the side of the hill, and we see that they all started from one straight line. To keep the boundary, stones seem to have been used, and fresh marked each year no doubt, as the line became soiled up. In process of time these lines upon lines of stones would result in a bank faced with stones, as we now find, and in flat ledges or terraces on the hillside.

These terraces, moreover, are often found associated with pit-dwellings, earthworks, and old trackways; and I have generally found near them many pot-boilers or flints, which when heated were used for cooking purposes. Hence, a very much stronger case can be made out for the artificial than for the natural origin of those terraces, as shown by Scrope, Seebohm, and others.

F. J. Bennett.

Salisbury, *July* 8, 1897.

SCOLOCYSTIS, ECHINOCYSTIS, AND *LYSOCYSTIS: DISCOCYSTIS, ECHINODISCUS,* AND AGELACRINIDÆ.

SIR,—In his valuable paper "On *Echinocystis* and *Palæodiscus*" (Quart. Journ. Geol. Soc., liii, 123–36, pl. vii, February, 1897), Dr. J. W. Gregory has a footnote (p. 133) that may, if not emended, give others as much trouble as it has given me. "To save further confusion" Dr. Gregory proposes the name *Scolocystis* for an obscure cystid to which the preoccupied name *Echinocystis* was given by Hall. We are not told what Hall, what species, or in what publication, although a mysterious thing called "Hall's date" is discussed. My friend Dr. Gregory kindly allows me to say that the reference is to *Echinocystites nodosus*, James Hall, "Account of some new or little-known species of fossils from . . . . the Niagara group," 20th Rep. N.Y. State Cab. Nat. Hist., p. 316, 1867, also published in advance as part of 18th Rep., December, 1864, and January, 1865, as well as on p. 360 of Revised Edition, 1870.

Having this information, it is easy to see that the acute Mr. S. A. Miller, in his most useful work, "North American Geology and Palæontology" (1889), to which it is the duty of every writer on American fossils to refer, has anticipated Dr. Gregory, and has there proposed the name *Lysocystites* for the genus of *E. nodosus*.

There is another generic name proposed by Dr. Gregory on p. 131 of his paper, viz. *Discocystis*: this, too, requires some comment. Dr. Gregory argues that *Palæodiscus* and *Echinocystis* are true echinoids, having no connection with the Agelacrinidæ. "There is, however," he says, "one genus which, if correctly described by Worthen and Miller, may be a Carboniferous representative of either *Palæodiscus* or *Echinocystis* [and therefore an echinoid]. According to the description of its authors, this remarkable form has an irregular, sac-like body; above the mouth there is a series of plates which may represent jaws [i.e. echinoid jaws]. The name *Echinodiscus* was given to this fossil by Worthen and Miller, but this term having been preoccupied by Agassiz for an echinid, for which it is still in use, the name of the cystid may be changed to *Discocystis*."

If Dr. Gregory considers *Echinodiscus optatus* (for that is the species to which he is referring) to be an echinoid, perhaps he will explain why he calls it a cystid and *Discocystis*. That it is a cystid and no echinoid at all, is my firm belief; and had Dr. Gregory been better acquainted with the writings of Mr. S. A. Miller, we should probably have had neither the above enigmatic paragraph nor the name *Discocystis*. At all events no one has any business to fling names about in this reckless way without considering all the facts and possibilities of the case. Three species have been referred to this genus, and should all have been reckoned with by my onomatopœic friend. They are: *E. optatus*, Worthen and Miller (1883), the type-species; *Agelacrinus kaskaskiensis*, Hall ("Geol. Iowa," i (2), 696, 1858), referred to *Echinodiscus* by S. A. Miller ("N. Amer. Geol."); and *E. Sampsoni*, S. A. Miller (17th Rep. Geol. Surv. Indiana, p. 76, 1891). Comparison of these shows plainly that they belong to the Agelacrinidæ. The "mouth," which Dr. Gregory innocently quotes,

is the anus, which in 1883 and even later was thought to be the mouth by many American writers. The supposed jaws are presumably the anal pyramid, which Dr. Gregory suggests came to be part of the apical system, i.e. at the aboral pole.

The species, in short, though perhaps not referable to the Carboniferous genus, *Lepidodiscus*, do not depart from the plan of the Agelacrinidæ, and anyone who wishes to maintain them in a separate genus should be asked to show the differences between them and *Agelacrinus*, *Haplocystis*, and one or two other of their allies.

Dr. Gregory appears to accept Haeckel's view that the ambulacra of the Agelacrinidæ bore pinnules. This may have been the case with one or two of the genera placed by Haeckel in *his* Agelacystidæ; but it can scarcely have been the case with any of the genera mentioned by Dr. Gregory.

It seems as well to indicate these minor lapses, which none will regard as detracting from the interest or value of my friend's long-looked-for paper.

British Museum (Nat. Hist.),
London, S.W.

F. A. Bather.

OBITUARY.

THE REV. ROBERT HUNTER, M.A., LL.D., F.G.S.

Born 1823. Died February 25, 1897.

Robert Hunter was born at Newburgh in 1823, and was the son of Mr. John Mackenzie Hunter, a Scotsman from Portpatrick, in Wigtownshire, his mother being (*née* Agnes Strickland) an Englishwoman from Ulverston, in Lancashire. In 1826 his father, with his family, removed to Aberdeen. There Robert Hunter, after attending the leading academy, entered the Grammar School, then under the celebrated rector, Dr. Melvin. Here he took the first prize in the third, fourth, and fifth classes of the school, and afterwards, when still only fourteen, he came out at the head of seventy-nine competitors in the open examination for University bursaries. He thus obtained the first, and entered Marischal College in the University of Aberdeen. There, a few months later, he was first in Latin and first in Greek, and in the third year first also in Mathematics. Among secular studies, however, Natural Science had supreme attractions for him. This subject was conducted by Dr. John Shier, and in his second year Robert Hunter gained the first prize, the second being awarded to Hugh Mitchell, afterwards Minister of the Free Church, Ferryden, who proved in later years a most excellent geologist and palæontologist. [The Rev. Hugh Mitchell, M.A., LL.D., passed away on November 10, 1894, and his obituary, written by Dr. Robert Hunter, his friend and former classmate (now, alas! also lost to us) appeared in the Geol. Mag. for 1894, p. 575.] Robert Hunter and Hugh Mitchell, with an ardour prophetic of future eminence, roamed the country for many miles around the granite city making Natural History collections. But, as the result of prosecuting researches in spite of wind and weather, Hunter was laid up for three months with a serious illness. The day of

competition for the Mathematical scholarship to be held for two years arrived, finding Hunter still confined to his bed. But the same indomitable pluck which carried him through numberless other severe trials in later life, stood him in good stead. He arose and entered the lists "pale and emaciated," but came out bracketed first with another student, with whom Hunter divided the spoil. At the end of the arts curriculum, these two young men were singled out to receive from the University the degree of Master of Arts "with honourable distinction." Hunter afterwards, in the Divinity Hall, obtained the Lord Rector's prize for Hebrew.

On quitting Aberdeen he went out as a tutor to the Bermudas, and determined to occupy his spare time in studying the Natural History of those islands. Having been introduced to Professor Owen by one of the Scotch Professors, he received particular directions for collecting corals, and preservative fluid in which to place the living coral-polypes for subsequent examination. In this task Hunter was most successful, bringing back, in 1845, excellently preserved examples of the "Brain-coral," *Meandrina cerebriformis*, with the animal, which Owen declared to be the finest specimens which (at that time) had reached this country. He was also placed in communication with Sir William Reid, the Governor of Bermuda, and through him was introduced to Sir William Hooker, of Kew, who evinced a warm interest in Hunter's botanical researches. At this time he might have readily obtained an engagement as a naturalist; but he was so strongly impressed by the conviction that his vocation was in the service of the Free Church of Scotland, that he once more resumed his theological studies.

Subsequently Mr. Hunter offered himself for the Foreign Mission Service, was accepted, licensed, and ordained, and in 1847 he was sent to be the colleague of the Rev. Stephen Hislop, at Nagpore (Nágpur), Central India. Hislop, like himself, was a born naturalist. For upwards of eight years Hislop and Hunter worked together in the heart of the old Mahratta kingdom, which was not then, as now, under British, but under native rule.

Whilst earnest in the missionary cause, in their walks and mission tours their keen scientific proclivities impelled them to take note of the geology, the fossils and the minerals, of the country. Thus we find them sending home papers to be read at the Geological Society of London—"On the Geology of the Neighbourhood of Nágpur, Central India" (see Quart. Journ. Geol. Soc. 1854, x, pp. 470–473, and 1855, vol. xi, pp. 345–383, pl. x); "On the connection of the Umret Coal Beds with the Plant-beds of Nágpur, and of both with those of Burdwan" (op. cit. 1855, xi, pp. 555–561). After 1855, the geological work in India wholly devolved upon the Rev. S. Hislop, although sympathized in most warmly by the Rev. Dr. Hunter here at home. "Tertiary Deposits associated with Trap-Rock in the East Indies, and Fossil-shells from those deposits" (op. cit. 1860, xvi, pp. 154–182, and pls. v–x); "On the Age of the fossiliferous thin-bedded Sandstone and Coal of Nágpur" (op. cit. 1861, xvii, pp. 346–354); "Supplementary Note on the Plant-bearing Sandstone of Central India" (op. cit. 1862, xviii, p. 36);

"Fossil Teeth and Bones of Reptiles from Central India" (op. cit. 1864, xx, p. 117 and pp. 280–282).

The fossil plants collected by them from Nágpur were described by Sir Charles J. F. Bunbury, in the Quart. Journ. Geol. Soc. 1861, xvii, pp. 325–346, plates viii–xii. A Labyrinthodont reptile (named *Brachyops laticeps*) was described by Professor Owen from Nágpur (Quart. Journ. Geol. Soc. 1855, vol. xi, pp. 37–39, pl. ii; 1859, xv, p. 647; and 1861, xvii, p. 353). Professor T. Rupert Jones described the Fossil Cypridæ from Nágpur (Quart. Journ. Geol. Soc. 1860, vol. xvi, pp. 186, 187, 189, pl. x).

"Hislopite," a new mineral, was designated after his friend by the Rev. Prof. Haughton (op. cit. 1860, vol. xvi, p. 160), and "Hunterite" by the same author, after Dr. Robert Hunter.

The effects of a tropical climate, however, began to tell upon Mr. Hunter's constitution, and in 1855 his health completely broke down, and he was ordered to start at once for Europe. Thus the personal association of these two ardent Scottish geologists and naturalists in India terminated, but their warm friendship continued until severed by the sad death of the Rev. S. Hislop, by drowning, on September 4, 1863, about twenty miles from Nágpur, in attempting to cross a river in flood after dark, when only in his 46th year.

Dr. Hunter continued to carry on the work of the Free Church Missions, preaching on behalf of the cause for India at home. He suffered the loss of a brother, also engaged in the missionary cause, with his wife and child, who were killed during the Indian Mutiny.

Taking up literature, the Rev. R. Hunter wrote for the British and Foreign Evangelical Review, and brought out in 1863 his "History of India." For two years he held the office of resident tutor in the Theological College of the Presbyterian Church in London, but resigned in 1866, in order to enjoy greater freedom in preaching and in the pursuit of his literary work. His contributions to the magazines were at one time most varied and prolific, but his "colossal work," as Sir Richard Owen called it, was the bringing out for Cassell & Co. the "Encyclopædic Dictionary," upon which he spent seventeen years of his life; the first seven years working entirely alone, but afterwards with assistance. This was followed by a manual of the Bible. He did splendid work with schools and a church at the Victoria Docks, the latter built for him by Mr. James Duncan. He was elected an F.G.S. in 1868, and the Senatus of Aberdeen University, in 1883, conferred upon him the degree of LL.D.

Those who (like the writer) had the good fortune to know the Rev. Dr. Hunter personally, must have been struck by his extreme modesty and simplicity of character, and his amiable and unselfish nature. Yet beneath this unassuming and retiring exterior there lay vast stores of learning which were at anyone's command who might ask his help.

He has bequeathed by his will a sum to be devoted to the working out and publishing descriptions and figures of the fossil plant-remains collected by him, and by his friend the Rev. S. Hislop, in Nágpur, Central India.—(Partly from *The Presbyterian*, Sept. 15, 1893, and March 4, 1897.—H. W.)